

# CRITICAL OBSERVATIONS

ON

BOOKS,

ANTIENT AND MODERN.

NUMBER IX.

VOL. III. An Introductory Discourse on the Abuse of the Talent of Disputation in Religion, preached at the Cathedral Church in Norwich, at the primary Visitation of LEWIS, Lord Bishop of the Diocese, in 1784. published at the request of the Clergy.

Researches concerning Chronology, continued, being an Enquiry into the Duration of the probable Age in Asiatic History, from the Commencement of the first Kingdom of Medes, to the End of that Empire at the Accession of Cyrus, including about 300 Years.

VOL. IV. A Preface to the 4th Volume, containing an Examination of Dr. Priestley's Remarks on my Discourse,

Remarks in Vindication of Justin, Epiphanius, and other Christian Fathers from the Mistakes or Misrepresentations of modern metaphysical Reformers of Christianity, who style themselves Unitarians; shewing, that there never existed during the first two Centuries, any Sect among the Christians, who considered the Messiah or Christ as being mere man, not even among the Jews; but on the Contrary, that all Christians believed, that Divinity was united with Humanity in Jesus, either at his Incarnation or Baptism: so that Unitarianism cannot have been original Christianity, but a later Invention.

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Vol. III. A. An Introduction, Discourse on the Abuse of the Tongue  
of Religion in Religion, preached at the Cathedral Church in  
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the Diocese, in 1784, published at the request of the Clergy.

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Christian Fathers from the Misapprehensions of the  
modern metaphysical Reformers of the Jewish, who have  
represented themselves as Unitarians; showing that the same existed during  
the last two Centuries, nay, still among the Christians, who  
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AN INTRODUCTORY  
DISCOURSE,

CONCERNING THE  
ABUSE OF THE TALENT OF DISPUTATION  
IN RELIGION BY ALL CLASSES OF MEN,  
PARTICULARLY  
BY THOSE FROM WHOM IT OUGHT TO BE NOW  
LEAST EXPECTED, WHO STYLE THEMSELVES  
RATIONAL CHRISTIANS, AND FREE  
ENQUIRERS AFTER TRUTH.

# DISCOURSE

ERRATA

In the INTRODUCTORY DISCOURSE

P. 13. line 25. *for* Divinity *r.* Dignity.

P. 16. line 28. *for* in *cœlo* *r.* *cœlitus*.

P. 17. line 2. in note, *for* in what follows the last sentence above quoted, *r.* near the beginning of his account of the Ebionites.

P. 29. line 15. *for* now, *r.* how.



A  
DISCOURSE  
ON THE  
ABUSE OF THE TALENT  
OF  
DISPUTATION in RELIGION,

Particularly as practiced by

Dr. PRIESTLY, Mr. GIBBON,

And others of the modern Sect of Philosophic Christians.

Preached in the CATHEDRAL CHURCH, NORWICH,  
at the primary Visitation of the Right  
Reverend LEWIS Lord Bishop of the  
Diocese, on June 23, 1784.

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Published at the Request of the Clergy present.

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By THOMAS HOWES.

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DISCOURSE

ON THE  
ABUSE OF THE TALENT

DISPUTATION IN RELIGION

By JESSE M. ALLEN

And others of the modern and orthodox Christian

Preached in the Church of the Holy Spirit, New York  
on the Twenty-Ninth of the Month of May  
The following is a list of the  
Persons who were present

Published at the Office of the City of New York

By THOMAS HOWES

Printed by J. and C. BARTHOLOMEW, No. 12, NASSAU ST.

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*First Epistle to TIMOTHY, I. v.*

*The end of the Commandment is Charity,  
out of a pure heart, and of a good con-  
science and faith unfeigned, from which  
some having swerved, have turned aside  
unto vain jangling.*

**I** NEED not mention to this intelligent and learned audience, that Charity, in the extensive sense wherein the sacred writers use the word, includes the practice of all the social duties of men to each other, not only as men, but as rational men, improved by civil and religious institutions, in order to render them more friendly fellow-creatures, and more virtuous fellow-christians: Now this better instruction of men in the whole circle of their practical duties, it was the very end and aim of the revealed commandment by Christ to promote, as St. Paul here in the text reminds us "The end of the commandment (says he) is charity;" and then he immediately proceeds to point out some of those chief qualifications of the mind and heart, which if they accompany our en-  
VOL. III. B deavours



deavours after the end of the christian commandment, will assist us in our *speculations* as well as *practice*, and thereby give to our acquisitions the summit of perfection. Of these qualifications he mentions three, namely, "That our motives and endeavours should proceed out of a pure heart, and a good conscience, and a faith unfeigned;" or in other words, That *first* of all the *intentions* of men should be sincerely directed to this object of promoting the end of the Christian Commandment, and not be perverted either by any views of self-interest or by any conceited notions of knowledge; but proceed as out of a pure and upright heart; *Secondly* that the *exertion of the faculties* of men should correspond with their intentions, that both the words of their lips and of their pens, as well as the works of their hands, should be conscientiously guided by the purity of their intentions: and also *thirdly* that both of those preceding qualifications should be directed by a clear *conviction of their heads* and understanding. The propriety of this admonition is evident; for purity of intention is not of itself a sufficient guide to action; because men may mean well, yet through indiscretion or ignorance may nevertheless act ill; therefore St. Paul adds in the text as a third and conclusive qualification, necessary to give force

force and effect to the former two, "that Christians should have a faith unfeigned," that is, a clear conviction of their understanding and of their rational powers concerning the nature and excellence of the Christian speculative doctrine as well as practical commandment, so far at least as they tend to promote those duties of men, comprehended under the word charity. Faith, belief and rational conviction are only different degrees of the same operations of the mind and intellectual faculties, founded on such competent evidence, as men are able to obtain in every case, such as they are generally guided by in life, and such as is suitable to the nature of a revealed religion: this then is plainly a third necessary qualification to promote the end of the Christian Commandment\*. Where any of these three qualifications are wanting in a Christian teacher or writer; where the intention of the

\* I understand the Greek word Πίστις as meaning *persuasion* or *conviction* of any kind, and not as restrained to the more limited sense of *Faith*: it is indeed generally, if not always, rendered by *Faith* in our translation; but this sense, I apprehend, was adopted by our translators chiefly, because they found *Fides* in the Latin vulgate: but neither the Latin nor English word seem sufficiently to express the extensive meaning of the Greek word. *Conviction* is that approbation of the mind arising from the fullest evidence, which it is possible for human rea-

the heart is not pure and sincere in its search after truth; where the reasoning of the tongue

son to receive, such as sensible evidence, by the eyes or ears, or by self-evident truths, or such demonstrative conclusions as are deduced from self-evident truths in conjunction with sensible ones, as in mathematics, and such sciences as are connected with them, i. e. astronomy, &c. ; which evidence is by all considered to be *certain* and *indubitable*. *Persuasion* arises from the next degree of evidence to that producing *conviction*; and is derived from *very probable* proofs only, such as in oratory, history and in those sciences, which the ancients called *στοχαστικας*, wherein we can only approximate to truth, and indeed in most of the concerns of life. *Faith* is both in popular language and also by Mr. Lock considered as the approbation, which the mind gives, not to truths of which it is capable to judge itself, but which it acquiesces in upon the credit and trust it has in some other person's evidence and knowledge. But there are no such distinctions to be made concerning the greek word *πιστις*, this equally and indifferently means every one of these kinds of approbation or persuasion; and not only every kind of conviction itself, but also still farther every kind of *proof* or *evidence*, which produces that conviction. Thus Quintilian shews, that it was used to signify every kind of *evidence*, which could be produced before a court of justice. *Αποδειξις* (*demonstratio*) est evidens probatio—per ea quæ certa sunt, *fidem* dubiis afferens, neque enim certa incertis declarantur: hæc omnia generaliter *πιστις* appellant; quod etsi propria interpretatione dicere *fidem* possumus, apertius tamen *probationem* interpretabimur. 5. 10. Demosthenes uses *πιστιν επιτιθεσθαι*, *fidem* dare, jurare to signify oaths, which are reckoned among the strongest kinds of evidence: and Aristotle employs *πιστις* to mean proofs even of an astronomic kind by ocular ob-

servations



tongue and pen is not conscientiously guided by such a purity of intention ; or where both of them are not directed by a clear conviction of one's own head ; the necessary consequence must be, that instead of convincing others to their edification, a man will himself swerve from the end of the Christian commandment, and be turned aside unto vain jangling and uninformative disputation; which tend only to pervert the cause of truth, and to perplex both himself and all others.

The justness of this admonition of St. Paul concerning the necessity of these three qualifications in the text to guard against the abuse of the talent of disputation, christianity

servations (*de Cælo. 2. 12.*) Quintilian, we see thought *Fides* of too limited a sense to express *πίστις*, why then should it be tyed down in Scripture to the still more limited sense of *faith*, without including all kinds of persuasion or conviction ? I fear, that both unbelievers and enthusiasts have drawn unwarrantable conclusions from the very restrained sense of our word *faith* : for while unbelievers on the one hand have contended hence, that the evidence of Christianity depends on implicit faith, not on argument or rational conviction; Enthusiasts on the other hand have equally contended, that a christian is able to atone for the badness of his actions by the strength of his faith, as if they thought themselves entitled to the most merit in believing, wherever they found the least evidence and conviction ; otherwise one cannot well conceive what merit they can claim from believing, where the evidence of itself, if fairly considered, cannot fail to produce conviction.

has

has had repeated cause to acknowledge in all ages from its beginning: It has sustained a variety of attacks from different quarters, which through want of some one or all of these three advantages in its adversaries, have been attended with no better success, than to convince the intelligent part of mankind, that though truth may be misrepresented, yet it cannot be altogether subverted by the perverse disputings of men, which the Apostle here styles vain jangling. To enumerate the different and opposite conduct of the several adversaries of christianity in different ages would require too much time; it is sufficient to observe in general, that having in the beginning of the present century been beaten out of all their pretences for absolute unbelief concerning the origin, nature and utility of the Christian Revelation, they seem now desirous, at the close of this memorable century, to adopt a kind of compromise with christianity; for they now affect to style themselves rational christians and philosophers: that is, they pretend to retain some parts of the Christian Revelation, such as they judge most proper; but to reject other parts of it; and thus they would form some new and peculiar Christian system of their own, differing however according to each man's own philosophic fancy.

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Just as the Electic sect of philosophers in prophane times adhered altogether to no one philosophic sect, but selected some of their tenets out of one sect, and some out of another, and thus formed a motley and incoherent mixture out of all: or just as the most antient Christian sect, called the Gnostics, intermixed Christian and Heathen opinions together; so in like manner our modern sect of rational Christian Philosophers pursue a similar plan, but with just as little success; and this because they daily prove themselves to want some one or all of those three qualifications mentioned in the text, either a pure *intention* of the heart to search out truth in their speculations; or else a *conscientious adherence* to the reality of facts and to the right sense of words to which they refer; or else a clear *conviction* of their own understanding concerning the origin and progress of christianity, the nature of its speculative doctrine or its practical tendency to promote the end of the revealed commandment. Hence accordingly this latest class of philosophic Christians is found to end in no more convincing and certain conclusions than the many others, who have gone before them, under the different names of Arians, Sabellians and such like: in which latest philosophic class Dr. Priestly has rendered himself the most conspicuous in his History of the

Corruptions



Corruptions of Christianity. But as this revived plan of the ancient Gnostics, in rejecting or admitting just as much and no more of the orthodox Christian tenets, than shall suit each man's own speculative opinion, could not be recommended to the public except by the aid of misrepresentation; so accordingly we find, that our modern philosophers have dealt as largely in this way as that more antient Gnostic sect, at the beginning of Christianity: the antient sectaries, we are informed by Eusebius, altered and corrupted the *words* of Scripture in order to make them suit with their own opinions\*; and the modern philosophic sect do much the same thing; for they alter the *obvious sense* of scriptural words, in order to make them favour their own conceptions. Some portion of genius however is necessary to render even misrepresentation itself plausible; accordingly this class of writers is found powerful enough in the faculty of perplexing a cause, in the art of words and the talent of disputation; the produce of which medley of ability and infirmity has too often a very imposing appearance in its favor with those readers, whose own inattention has disabled them from discerning the wide difference, between the talent of separating

\* Hist. Eccl. 5. c. ult.

truth from falsehood, and that of confounding both together. The investigation of truth is a slow paced labour of patience, which gains its object only by degrees ; but the art of confusion proves most successful, when like a rushing muddy torrent, it can level all distinctions and boundaries in a moment ; and of course by thus disdaining all confinement, it cannot but often turn aside from those three qualifications necessary to be attended to by a teacher of mankind, the intention of the mind, the truth of one's assertions and facts, and a clear conviction of the head concerning the necessary conclusions resulting from any argument. Yet this abuse of the art of language and reasoning, though a misfortune, is a misfortune incident to human nature only in common with the abuse of all other blessings of our existence ; birth and wealth and strength, commerce and government, power and liberty, all social and all liberal arts are liable to the same abuses ; and by the possessors of them swerving from propriety in the application, they may be diverted from the beneficial ends intended by such human advantages. This misfortune arises from the frailty of our nature, so that the very same means, the same art of words and persuasion, by which Christianity was at first

preached and established, may be now employed to perplex and undermine it: but intelligent men will discern the difference between the right use and the abuse of any human qualification; to define exactly the limits between the two is impossible, yet there will generally be some inherent strong marks, whereby they may be known; "by their fruits ye shall know them."

With respect indeed to the *intention* of the heart, this by being latent within, is the most difficult to discover, as well as the most disagreeable to display to others; yet even this will in some degree be manifested of itself by its effects, just as an intent in law is manifested by the overt acts which follow it. Hence therefore wherever we meet with continual misrepresentations in any writer, and these all leaning the same way, all in favour of a man's own cause, how can any one fail being tempted to conclude, that these must have been intended, and not be the effects of mere over-sight and accident? The same again when every fact or testimony is omitted, which makes against a writer, and those only produced which are favourable to him; or when we often meet with a more than ordinary insolidity of reasoning in those same persons, who on other occasions reason with justness and accuracy;

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in such cases candor must arrest its favourable judgment of a man's intention, and even Christian charity itself, which teaches us to hope all things and to believe all things in favour of others, must then, I fear, stop short at hope, without advancing to belief.

So also, in regard to the *second* article of a *conscientious exertion of one's faculties*, had those writers in every age, who have turned their philosophic enquiries to the nature of the Christian doctrine concerning the deity, had they kept in view as they ought, the admonitions of St. Paul in the text, they would not have abused, as they too often have, the gracious gifts of God, by turning the arts of language and reasoning into mere arts of perplexed disputation and vain jangling. Thus for instance among the many philosophic disputants, who have succeeded each other since the commencement of Christianity, two of the most conspicuous classes (as I said before) are those of the ancient Gnostics, and our modern rational Christian Philosophers, whom I may call a modern sect of Gnostics; for they have both affected the same style and title in pretending, that they alone of all men deserve the name of clear sighted *learned* men. That ancient sect of Gnostics, being struck with admiration at the rational notions concerning

the Deity in the Christian Religion, as well as its solid morality (both which have indeed been the admiration of every infidel ever since) yet being deluded by a conceited opinion concerning their deep learning and philosophy, they pretended, that the orthodox Christian doctrine concerning the divinity of Christ was not sufficiently sublime; they undertook therefore to amend and improve it by the addition of philosophic opinions of their own, in order to render it more complex and remote from all vulgar notions, and more suitable (they thought) to a *merely speculative* turn of mind. Our modern Gnostics have manifested a similar inclination to alter and amend the orthodox tenets of Christianity, especially with respect to the divinity of Christ; and from a similar motive, a pretence of correcting vulgar notions, in order to render them more conformable to their own *speculative* refinements, rather than to any greater *practical* use, but which they dignify however with the title of rational and philosophic improvements: while in fact the pretended corrections by both sects amount to nothing better than doubtful conjectures of their own, in opposition to the plain evidence of words and facts in scripture, according to the best critical interpretation of the Greek language now within our power; and  
this

this confirmed by its being agreeable to the sense, which (as we find) was put upon the same scriptural phrases, while Greek was a living language. In one point however the ancient and modern sects have differed, although they agree in so many others: they differ in this, that the ancient Gnostics endeavoured to enoble the dignity of the Christ in a manner, they thought, more consistent with divinity than among the orthodox themselves; whereas our modern Gnostics of the present times take the opposite road, and seek to abase the person of Christ, to deprive him of all divinity and reduce him to a mere mortal man. Now for confirmation of this their opinion concerning the mere humanity of Christ, the latter pretend to appeal, not merely to the words of Scripture, but also to the belief of that ancient sect of Gnostics and other Christian sectaries of the first ages; than which a more ill-founded misrepresentation was never attempted to be offered to the public: for, as I observed before, the ancient Gnostics and other sects sought to exalt the divinity of the pre-existent Christ higher than any others, and neither they, nor in fact any one Christian sect whatever of the first ages, ever held any such opinion as the mere humanity of Christ; I mean in that sense, in which it is understood by our modern



modern Gnostic-Philosophers, that is, as if humanity extended throughout the whole life of Jesus, from his incarnation to his passion. What the Gnostics held was this; they sought to exalt the dignity of the Christ; and they conceived it to be an abasement of that dignity, for the pre-existent divine Christ to be incarnate and born like man; therefore they pretended, that Jesus was indeed mere man from his incarnation to his baptism; but at that time by the descent of the Holy Spirit of God, an union was formed between the pre-existent divinity of the Christ or Savior and the humanity of Jesus; which was dissolved again, as they supposed, and for a similar reason before his passion. The very same likewise were the notions of every other antient Christian sect, which in any respect whatever held the mere humanity of Jesus; namely that this separate humanity continued only for a time, that is, until his baptism and no longer; and there is no sufficient proof extant in any author whatever, that the Nazarenes, or Ebionites differed in this respect from the other Christian sects, as may be easily proved to the satisfaction of any impartial man; nay indeed it is expressly pointed out by Epiphanius, that the opinion held by these sects concerning the humanity of Jesus was either nearly or exactly the

the same with that of the Gnostics. If this fact be true, then it is plain, that all these sects differed from the orthodox chiefly with respect to the time, when the union of divinity and humanity took place, not with respect to the fact itself and the universal belief of the reality of such an union, as well among the Christian sectaries as the orthodox; the latter conceiving this union to take place at the incarnation, the Gnostics and other sects not until the baptism of Jesus.\* Notwithstanding this our philosophic Christians have not scrupled to affirm and have repeatedly

\* I use *Orthodox* in opposition only to *sectary*, and do not mean the particular tenets of any present established Church. The evidence on which the above assertions are made will be collected more at large in the 4th vol. of *Critical Observations on books antient and modern*, sold by B. White, Fleet-street. At present it may be sufficient to observe, that with respect to the Gnostics it is proved by almost every chapter in Irenæus, that they held the divine pre-existence of the Christ and Saviour and his union with Jesus at Baptism: that the Nazarenes and Ebionites held nearly the same tenets will be plain from the following passages in Epiphanius. Nazaræos Ebion proxime consequitur, *eadem que cum illis dogmata tenuit; quod horrendum monstrum et multiplex*——*rursus ad exitium humani generis erupit: licet ex illorum disciplina prodierit, nonnulla tamen, præter-quam ab illis doceantur asseruit, &c.* In giving an abstract of the contents of the Gospel used by the Ebionites, he says it contained to this effect “Cum ascendisset (Jesus) ex aqua, aperti sunt coeli et vidit spiritum dei sanctum columbæ specie descendentis et intrantis in sese, et vox reddita de cælo  
dicens,

dicens, *Tu es filius meus dilectus, in te mihi complacui*, et iterum, *Ego hodie genui te* [ἐγὼ σήμερον γεγεννηκα σε]—nunc ad inchoatam de Ebionitis narrationem regredior—Jesum, ut diximus, *merum hominem* existisse putant, in eoque resedisse Christum, qui columbæ specie sit illapsus, cum eoque conjunctus [ἐν εἶδει περιστερας καταβιβηκοτα και συναρθεντα αὐτῷ] id quod aliis insuper Hæreticis placuisse reperio. Ita Jesum ex utriusque parentis femine progenitum ipsum devenisse Christum. Rursum vero negant ipsum esse *hominem* ex responso salvatoris “Hi sunt fratres mei et mater mea, qui faciunt voluntatem Patris mei.” Hinc ille præstigiis instructus Ebion varias se in effigies deformat, ut esse monstrum quoddam appareat sicut ab initio dictum est. By Epiphanius’s giving at the beginning the same name of *monstrum* to those tenets, which Ebion held in common with the Nazarenes, it should seem, that the union of the pre-existent Christ with Jesus at his baptism was one of those which were common to both sects; he thus goes on. Duos a Deo constitutos asserunt Christum et Diabolum; Jesum vero electum fuisse, itaque dei filium in Electione appellatum, quia Christus in eum delapsus est columbæ figura. Negant vero a deo patre ipsum fuisse genitum sed creatum [καὶ φασιν οὐσι γεγεννηκοτα ἀλλὰ ἐκτισθαι] sicut unum ex Archangelis, majorem autem illis existentem, et *dominantem* [κυριοντα] super Angelos et omnia a creatore formata—Qui Ebionitæ vocati sunt, secus ac Ebion ipse, nescio quam in cœlo virtutem ex Deo obtinuisse Filium asserunt. *De Hæres.* Now let readers determine as they please concerning the degree of divinity, which Ebion attributed to the Christ either before or after the union with Jesus at baptism; yet thus much is clear, that he did not consider Jesus as being *mere man* any longer than his baptism: and there is no reason to think, that this was not likewise the tenet of the Nazarenes. Another article also appears equally clear from Epiphanius, namely that the later Ebionites after Elxai went still farther, and maintained, that Jesus was born mere man of Mary only; for that the Christ had descended and become united with Adam and several



repeatedly exerted their pens to maintain, that not only the first Christian sectaries disbelieved in the divinity of Jesus, but also even the greater part of the primitive Christians themselves; and hence they have sought occasion to accuse orthodox teachers as corruptors of Christianity. If we could suppose, that they knew they were asserting a fact, which was not true, and that they purposely

several others, and by means of the holy spirit of God with Jesus also at his *incarnation*, as is related in what follows the last sentence above quoted, and is confirmed by the account of the Ebionites in Eusebius. As to Artemon, Theodotus, Beryllus, Photinus and those called by Epiphanius *Alogi* in general, they were all followers of the Gnostics and Ebionities with some variations, in metaphysical expressions at least, more perhaps than in substance, and we find the same tenet of an union of the Christ with Jesus pervading them all. Epiphanius begins his account of Theodotus, by saying that he was a *shoot* [αποσπασμα] from the heresy of the Alogi: for which reason he enters but little into his opinions, having before often stated the *common* opinions of all those sects. But in the extracts from the doctrine of Theodotus made by Clemens of Alexandria and published along with that author, we there find clearly, that Theodotus also held the pre-existence of the Christ and his union with Jesus at Baptism, like the others: and indeed all, who held this doctrine, of necessity supposed the Christ to be divine in some degree or other both before the baptism and after; so that the modern opinion concerning the humanity of Jesus through life, has not the least countenance in its favour from the tenets of any one of the antient sectaries, how much less then from the antient orthodox Christians?

misrepresented the evidence concerning it; this conduct would verify the importance of the Apostle's advice concerning the necessity of that *second* qualification, of a *conscientious exertion of our faculties* to preserve ourselves free from dissimulation or concealment, when not only the truth, but the whole truth ought to be produced: and it would at the same time serve as an example, how often truth at the last comes out, and makes a just distinction between the use and abuse of the talent of disputation. However, I make no such presumption, and am sincerely convinced, that our philosophic sect had no knowledge of the above mentioned circumstance in the belief of the ancient sectaries, viz. that they confined the humanity of Jesus to the time of his existence before his baptism; but they certainly ought to have known it, and also to have been very certain, that they knew every thing concerning the subject, before they proceeded such lengths, as to rank orthodox teachers among the corruptors of Christianity. Although this error may have arisen from a hasty inattention in those who at first proposed it, yet it must be a less pardonable inattention in others to have adopted it without sufficient examination, as appears indeed but too plainly both in this and many other instances;

stances; and less excuseable still if those examples of indiscretion should induce future writers of the same class to overlook the mistake on purpose, and thus all become assistants in some degree toward misleading the public and turning perhaps many from Christian belief of any kind.

This mistake then of theirs, proves the propriety of attending to the *third* qualification recommended by the Apostle, a very extensive knowledge of the head, and a clear *conviction of the understandings* of men concerning all the speculative doctrines as well as historic facts relative to Christianity, before any of them be brought to public censure: it will also serve as a specimen how often by the abuse of the talent of disputation, truth and falsehood may be confounded together even by those who do not intend it. For indeed what a superstructure of error have our philosophic Christians built upon that above mentioned mistake of theirs as the foundation; the whole of which falls to the ground at once, so soon as the foundation is taken away, by rectifying their misconception of that single fact concerning the humanity of Jesus? If, for instance, they pretend to find any difficulty or matter of objection against the belief of an union of divinity and humanity in the person of Jesus; yet they



do not in the least remove the supposed difficulty by having recourse to the creeds of the antient sectaries, those favourite, pure and primitive Christians: for they all believed in the same union; and the difference of time when they supposed that union to take place, cannot possibly make the least difference in a conception of the reality of the fact itself. Moreover, if it were actually true (as they contend) that a belief in the humanity of Jesus had been as universal among the first Christians as they pretend; yet if this also was restrained to the time before the baptism of Jesus, it can give no assistance to their own cause in maintaining the mere humanity of Jesus after his baptism: nay on the contrary by its thus appearing, that the whole tide of belief among excommunicated sectaries themselves was in favour of the divinity of Jesus after his baptism; this becomes a certain proof, that no such opinion as that of the mere humanity of Jesus during the whole of his life, could have possibly prevailed among the orthodox Christians of those ages. And still farther, if appeal to the antient sectaries, concerning their supposed belief of the humanity of Jesus after his baptism, be brought forward by themselves as a proof of the same belief among the orthodox, surely, they will not be so inconsistent, as now to change

change fides, and (since we have proved that those sectaries believed in the divinity of Jesus) refuse to admit this as any proof of the belief of the most ancient Christians in the same fact. How much insolidity of reasoning then and how many false conclusions have followed, through an indiscreet abuse of the talent of disputation, from the misconception of a single fact; which requires nothing more to rectify it than to read Ireneus's account of the Gnostics, Epiphanius's History of the Ebionites, and the extracts from Theodotus made by Clemens of Alexandria! If indeed any one should read only Augustin's Epitome of heresies, he might at first be inclined to conclude, that several of the Christian sectaries maintained the humanity of Jesus throughout the whole of his life: but it should be considered, that Augustin's brevity, by contracting often times his account of a whole heresy into a single sentence or two, necessarily led him to notice only some one striking feature of each sect, which distinguished it from the orthodox. If then a reader shall have recourse to the above-mentioned other writers, who have treated of the tenets of those sectaries more in detail; he will find that the Gnostics, Nazarenes, Ebionites, Theodotians and all their followers down to Photinus, had no conception of ever extending

ing the mere humanity of Jesus to any later period than his baptism: and that the same principle pervaded them all, namely that of enobling, not abasing the dignity of the Christ; although indeed they modified it in several different ways, or at least delivered their sentiments in such a different manner, that if they themselves had formed no clear distinctions in sense, yet at least they did distinguish in metaphysical expressions. Now do these examples afford any favourable specimen of the critical, historical, or argumentative attention of our moderns, or any good presumption, that they would be able to interpret the words of Scripture itself with greater accuracy, than they have displayed in these instances? or with more critical knowledge of language than orthodox teachers, or better than those ancient Christians themselves, whose explanation of scriptural phrases have been transmitted down to us, and were made at first while Greek was yet a living language? Upon the whole does the modern philosophic sect appear to have any good reason to claim, that a clearer *conviction of their heads and understanding* is attached to the exertion of their own pens and faculties than among orthodox teachers? We may surely at the very worst be able to say with the



the Poet *Iliacos intra muros peccatur et extra.*

In truth I am inclined to think, that the *policy* of our modern Gnostics is preferable to their accuracy, *to the conviction of their own understandings*, and to the force of their arguments against others. For they being willing to retain some parts of the Christian Doctrine, while they reject other parts; but being not willing to expose themselves to the odium of presumption in rejecting any portion of the Gospel, and directly setting up their own philosophic speculation as being wiser than Revelation itself; they could find no more specious method to secure their own opinions from public censure, than by denying the opinions of the orthodox to be contained in the words of scripture, or to have been believed in by the primitive Christians: and they could find no better method to prove this, than by giving a different sense to the words of scripture from that in which they always have, and ought to be understood according to the most critical skill in the Greek language; and also by greedily catching hold of the supposed state of belief in the first Christian ages, and among the first Christian Sectaries as an argument in their favour. This may be political indeed, but is it not unjust to one's neighbour? Is it candid

candid and impartial? Is it consistent with the end of the Christian Commandment, or with sincere enquirers after truth as they profess themselves to be? or is it not rather the specious refuge of such, as manifest an *intention* to turn the art of reasoning into a wilderness of disputation, in order to lose truth in the midst of it, instead of helping us to find it? whereby they become the better able to conceal their own presumption in attempting to reduce christianity to a philosophic system; by so puzzling and perplexing the cause, that superficial readers of modern times may not perhaps be easily able to find a right path through the labyrinth. But is this the part of men of real knowledge, who are to mend the understandings of us all, and to mend Christianity likewise? whose profound speculations profess to deal out Christian truths never heard of in the first Christian ages; and who boast of being able to give light to them, that now sit in darkness, in the shadow of death and religious corruption. Alas, true philosophy teaches a different lesson, and true philosophy does indeed demand the respect of all reasonable men; but it no longer deserves that name, than while it pursues its proper object, that of leading us to more certain knowledge and wisdom than we possessed before, and not merely of exchanging one doubtful truth for another :  
when

when directed to its true object it will teach us, that in all our investigations concerning the nature of the divine being and Christ man's reason is soon lost in the immensity of the subject, and must at last repose its deepest speculations in the bosom of a rational humility; it may therefore just as well begin with a Christian humility, and this would have led towards candour and justice to the Christian labours of others. For between Christian and philosophic humility there is this difference, that the Christian begins his religious studies with it; but with the same humility the pride of the most sublime philosophy, if it proves to be indeed rational to the very last, must end; it must end with the same humble acknowledgement, that the nature as well as the ways of God are so much above the understanding of man, that they can be nothing better learned from philosophy than from what is written in the Revelation of God himself. But to suppose, that this Revelation was never rightly understood even by those who heard it from the mouths of the Apostles themselves or their immediate successors, is to suppose, that the divine spirit of truth, which promised to lead Christians into all truths, did its work so imperfectly, that it is reserved for the spirit of modern philosophy to set matters at rights



again: which even if it could be supposed without shaking the whole authority of Revelation, yet at the same time it excuses orthodox Christian teachers for having retained those original Christian errors. Our modern Gnostics then might just as well have asserted explicitly at once, that though for decency sake they place those errors to the account of Christian teachers, yet the defects, of which they complain, are defects in the original plan of Christianity itself; and which our modern Apostles have undertaken to correct. But it was plainly the business of Christian teachers only to preach that doctrine most obviously taught by the words of the antient Gospel; which was received as the true sense of them in the first and purest Christian ages, which may be even at present traced back to the earliest times in the writings of Christian authors still extant, and which is confirmed by history as well as by a critical skill in the Greek language: for the investigation of the true doctrine of Christ is an historic and critical rather than a philosophic subject of enquiry. It was the business of Christian teachers only to make known what Christianity actually was at its first publication by Christ and his Apostles; not to enquire what may be now made of it according

ing to any man's opinion in order to suit his own particular taste ; nor yet what our rational Christians may think it ought to have been, in case their philosophic advice had been consulted. Christian teachers became the historians of this revealed religion, and in order to discharge their trust faithfully, they were forced to confine themselves to the truth of facts, and to the sense of phrases, as they actually found them to have been understood by the earliest Disciples of Christ: had they taken upon themselves to adulterate Christianity with opinions foreign to it; and as Philo-Judæus did in the Mosaic religion, to assimilate it to the Greek Philosophy, so that the Jews themselves could no longer know their own religion, when exhibited in his writings; Christian teachers might in that case have been justly accused of having handled the word of God deceitfully; and our philosophic sect themselves would have been the first to make the accusation. If then there be any tenets, which philosophers can reasonably consider as errors still inherent in reformed Christianity, yet candor ought to acknowledge, that they are so interwoven with the very body of it, or at least arose in so early an age, that it was impossible for Christian teachers to distinguish them with certainty, much less to separate them from

original Christianity. But even taking Christian belief as it now is and has been, whether the good ends already produced by it and which it is daily producing more and more, would be promoted and not rather retarded by any new reformations derived from Philosophical Theory, this is a matter too full of danger in practice to induce prudent men to try the event, and too full of doubt and difficulty even in speculation to induce reasonable men to attempt it. For who indeed can hope to collect such a new Christian system out of the Gospel, as shall be able to give better satisfaction to the different classes of men, if they do but attend to the fate of all the philosophic reformations of orthodoxy by the Arians and others since the beginning of Christianity ; none of which have long survived their respective authors, or been received with satisfaction by posterity ? But thus much all men may see most clearly, that if any one shall compare the temper of modern times in war or peace, in the principles of commerce, society or legislation, with the manners of the most civilized Ancients in prophane ages, they will appear to differ as much as two different climates, or as the roughness of winter differs from the mildness of summer : to what can this be ascribed but to the benevolent influence of the Gospel



Gospel in its present state with all its infirmities ; which has been now so long about our path and about our bed and so intermixed in all our ways, in our thoughts and our institutions, that through the medium of religion it has by degrees brought men to the use of reason. Men of experience then and practice in the concerns of life will perhaps know where to stop, although speculative philosophers should not ; and while they find themselves so well at present, while they thus feel so many good effects, they will look back with comfort on that lamentation in Xenophon concerning the unruly wills of mankind in antient times. “ Alas now much more difficult is it to govern man than all other animals ! ” New systems of Christianity therefore will be probably left to those who dignify themselves with the title of rational Christians ; but who at the same time prove themselves to be inconsistent philosophers, while they thus apply the speculations of reason to the subject of divine truths, concerning which no man can possibly obtain any knowledge by the use and conviction of reason only without the information of Revelation ; and this was the very cause why the antient philosophers in prophane times ran into so many extravagant notions upon the same subject : Can the Moderns hope to do

do better in the present age of inconsistency, of visionary reformations, metaphysical chicanery and popular delusion; which may probably produce as many bankrupts in Philosophic Theories of Christianity, as we daily see produced in commercial and political speculations? So long then as we confine ourselves strictly to teach the words and sense of scripture, as they have been fixed by the doctrine received in the earliest ages, we cannot but be thus far right; and even if the whole system of Christianity itself could be proved in the wrong, yet we ourselves at least cannot, who are only the faithful teachers of what the nature of that revealed law was at its first propagation, and not the formers of it. In the execution also of this our task we are employed upon a subject, which happily is within the compass of human understanding, that is in the investigation of the fact itself, what the doctrine of the revealed Gospel was at its origin and whether it be contained in the words of scripture; the latter is to be learned from the Gospel itself and a critical skill in the Greek language, the former from history and the earliest Christian writers still extant; in these points no man can fail to arrive at a sufficient *conviction* of his intellectual faculties, in case he does but make a *conscientious use* of them; and

and does not first hastily take up new opinions, before he has duly examined them, and then *intentionally* make all the testimonies of ancient writers bend to his own erroneous preconceptions. We see therefore to what degree the *conviction of our understanding* can arrive on these subjects, that it is founded on the most rational principles of investigation concerning *facts*, and the testimony of revelation concerning *truths*, not upon philosophic speculations concerning the internal nature of those truths respecting the Deity; for indeed the things of heaven who can or hath searched out? In the last point Christians, Philosophers and all must equally submit either with a religious or with a rational humility, as being a subject equally above the understandings of us all, in whatever different manner it shall be modified. Now on such a review, if the testimonies to the facts of the Christian Revelation, and if those articles of its doctrine, which are within the compass of our reason, shall appear to be of such a rational kind, as to justify our *faith* in those revealed truths which are above our understanding; we are at the same time *certain* also that no man can possibly arrive by mere speculation at any *conviction of his understanding* to the contrary of those divine truths thus revealed to us.

It



It is therefore to be much lamented, that any abuse of the talent of disputation, notwithstanding St. Paul's admonition in the text, should still subsist concerning religious subjects in this enlightned age, and one which is truly philosophic with respect to all merely human subjects of knowledge, so that neither the more rational temper of the times, nor yet the admonition of scripture should have rendered enquiries concerning religion conformable in fact to that spirit of truth, which is too often only professed in words. This doubtless arises from the too great warmth of literary zeal; but as scripture teaches, that the wrath of man worketh not the righteousness of God, so neither can a momentary display of an artful talent at disputation tend either to promote the end of the Christian commandment or ultimately of a man's own satisfaction, whenever it shall deviate from truth. For indeed what a poor reward is the vain glory of an aptness at perverse disputings in recompence for the long and painful cultivation of that particle of human reason, which distinguishes us from Savages! What an unworthy application is it of superior capacity and learning in any man, to be able to make a bad cause a good one, and to turn the best cause into a bad one, by confounding the understandings of men!

men ! If no better effects have often arisen from the superior parts of many writers, it is on account of their having made a shipwreck of all by swerving from those qualifications in the text of pure intentions of the heart in the search of truth, a conscientious conduct in the exercise of their tongues and pens, and a clear conviction in their own heads : but when assisted by these qualifications truth shall be indeed their aim, and sincerity shall guide their words, then the rest of their fellow-creatures must ever receive some profit either from their labours, or at least from their errors. Life itself also is too short, the business of it too urgent, the duties of life too many, and its comforts too few, for any of us to throw away the precious moments of health and time in perplexed disputes and vain jangling. When daily experience proves, that there are still so many truths to learn of importance to the welfare of man and within the compass of our capacities, yet so little leisure to be stolen from the necessities of our being, or from the calls of our several stations ; with what disapprobation ought men to receive the attempts of those, who seek to throw additional clouds of perplexity over any of the first principles of reason or religion ? What a mortifying reflection after all the labours of

others added to our own, to have obtained no clearer view concerning the objects of our researches, than our ancestors had long ago, instead of being enabled through time to emerge more and more from the darkness of doubt and ignorance! By the lot of humanity we rise by slow degrees from a tedious *Childhood*, in which knowledge indeed is pleasant, but study is painful: even in the next stage of *youth* knowledge must come in the shape of amusement, or else it will come in a questionable shape: and when arrived at *manhood*, both necessity and inclination urge us on to the active scenes of life; so that the silent hours of intellectual meditation are too often postponed to some future distant day, which perhaps never does arrive at all. For our period of maturity, like the sun ascended to its meridian height, passes on with such rapid haste, that we find ourselves far down the hill upon the *decline* of life, before we even think of where we are: Connected also as we must be with worldly cares, we must contract in return some attachment of interest for the objects of those several cares; and either domestick concerns, or business and society, either the conversations of the friendly world, the amusements of the polite, or the disputes of the political world, some or other will in every stage intervene too often,



often, and rob us of too many of those vacant hours for study, which the necessary demands of life could really spare; and thus we are hurried on along the common stream, until at length we feel with sorrow, that the best parts of *life* itself are *already past*, and then we think it too late to begin a new course of conduct. So that all our lives we can but just hold our heads above the surface of knowledge, at the very moments, when we most want a very elevated and extensive view of it for our help. Whilst then there are so many avocations through nature or fashion, so many impediments to knowledge from the necessary course of things; and while these are augmented by the inconsiderate conduct of the world, which when it claims at our hands both education and learning, yet never considers the time or expence required for either; and hence after having spent one fortune to obtain what the public demands, we find ourselves at last not possessed of fit worldly qualifications to acquire any other, at least in such an age as this, which expects a sacrifice of all things to gratify the capricious humors and self-importance of wealthy ignorance: whilst there are thus so many discouragements on all sides to the acquisition of learning, Oh! let not those, who are blessed with

with genius or leisure, add still another and a voluntary obstacle in our way, derived from their own conduct alone, by perplexing the road to intellectual improvement, with unsolid principles, unfair representations, improbable conjectures, and unjust conclusions; in short with all the tricks of the mere talent of disputation, as if they wished for a return of the ages of ignorance. But rather let all, in seeking to advance toward the regions of intellectual light either in reason or religion, remember, that the end of the Christian commandment is connected with practice more than speculation; and that both of these will be best promoted, by ever keeping in our view the admonitions of the Apostle in the text, concerning a pure intention in search of truth, a conscientious exertion of our tongues and pens during the search, and a clear conviction of our understandings, whether or not in regard to every subject it be within the power of human reason to attain to truth, or within the bounds of Christian humility to attempt it by the aid of reason only.

*P. S.* Left too much brevity should have rendered my meaning obscure in the first line of the note at p. 17, enlarge the sentence thus, "and that by means of the Holy Spirit of God Divinity was *possibly* in some degree united with Jesus at his incarnation, but certainly at his baptism."

F I N I S

*Mistaken*

CRITICAL OBSERVATIONS

ON

BOOKS,

ANTIENT AND MODERN.

NUMBER XI.

TO BE CONTINUED OCCASIONALLY.

CONTAINING

An Enquiry concerning the Duration of the celebrated Assyrian Empire, shewing by a Variety of antient Evidence, that Herodotus was accurate in dating the Foundation of it by Ninus later than the Trojan War: That the longer Period of 1460 was only formed by the superstitious Imposition of Asiatic Astrology, because that Empire was destroyed near the End of one astronomic Period of 1460, in the 120th Year before Nabonassar, and the Commencement of another Period at that Date. Hence the Astrologers pretended, that the above Empire had commenced at the Beginning of the preceding Period of 1460; it being a Maxim with them, that Kingdoms, like human Lives, were governed by the Revolutions and Conjunctions of the heavenly Bodies. It was afterwards reduced to 1300 Years by the same Greek Chronologers, who cut off nearly 160 Years from the Antiquity of the Trojan War.

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RESEARCHES  
CONCERNING  
CHRONOLOGY  
CONTINUED.

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*Connexion of the present Subject concerning  
the Commencement, Duration, and End of  
the Assyrian Empire, with our preceding  
chronologic Enquiries.*

LIFE is too short, and the avocations  
in it too many for any individual to  
expect sufficient time and leisure to form a  
complete system in any science, more es-  
pecially in such a complex subject as chro-  
nology, which necessarily embraces a large  
portion of the whole circle of sciences; I  
shall therefore go on to include what has oc-  
curred to me on that subject under the general  
title of *Observations* only; intending no  
more than to point out to future compilers  
of chronologic systems some of the chief  
places, where their predecessors seem to  
have taken the wrong road, although a  
better and plainer one was before them;

VOL. III.

G

where

where they have hastily adopted principles without good foundation, and drawn conclusions, which tend to produce perplexity and contradiction between prophane and sacred authors, instead of reconciling them as they proposed; while at the same time those chronologers have either overlooked or rejected other principles and testimonies, which have both more solidity and tend also to greater harmony. Having then agreeably to this plan laid a *foundation* upon solid ground in my *first* volume, and in my *second* sketched out in a table a *general view* of that chronologic system which results from my researches, as being apparently both true and harmonious; and having moreover occasionally delineated in some *particular* examples the harmonious effects it produces both in rendering prophane authors consistent with each other, as well as with sacred scripture; I propose to proceed by degrees in establishing the other component parts of this new system in the same minute and *particular* manner, as I have already traced back the dates of events from the invasion of Xerxes up to the accession of Cyrus; more especially with respect to those articles in which my proposed system differs chiefly from all former ones: when a few  
of



of the principal points are once thus fixed securely, to serve as guide-posts during our progress, it will become easy for others to fill in all the intermediate periods with their respective events in right chronologic order. Some readers, however, may think that I have been too minute, have often dwelt too long upon some of these principal points, and surveyed them on too many sides and in too many lights. I am sorry to be obliged to differ from this opinion; on the contrary it seems to me to be this very circumstance which contributes more than any other to recommend the present system, namely, that it is founded on principal dates so accurately and minutely traced out, that it will bear to be viewed on different sides and in different lights, and yet be found harmonious and consistent in all of them. It has been the opposite conduct which has been the cause of so many discordant opinions in chronology; for whenever a writer takes up any favorite hypothesis and wishes to recommend it, he has nothing else to do but to run over prophane and sacred writers, and it is scarcely possible but that he must meet with some evidence or other which may seem to render his own opinion plausible; if he stops here, he may find

equally good proofs for directly opposite opinions: but if he goes further and examines the consequences of every opinion, when compared with others; if he finds that one leads to inextricable difficulties and incoherences in some ages or reigns either in earlier or later times, while another leads to harmony every where; and that this is confirmed also by a great variety of different kinds of evidence, drawn from different authors and subjects no way connected with each other, and which could not without the greatest improbability have all thus conspired together in harmony by mere accident only, nor through any other cause than that of their being all founded on and derived from truth; it is this universal harmony and conspiring evidence, drawn from all quarters to one and the same date or dates, which can form the only solid ground-work for a system in chronology. Moreover, a system thus formed will often reciprocally become a good critical expositor of the meaning of many passages in antient authors both sacred and prophane, where the mere words themselves may be too brief or too ambiguous to determine the real sense of the writer; nay, it will form, as I have had already and shall have repeated occasion to shew,

one

one of the best and safest vindications of the veracity of sacred as well as prophane authors in a variety of cases. It is then for this latter reason as well as the former, that I enter into a minute detail in many cases, the subject of chronology often serving only as a thread, agreeably to my proposed plan, for connecting together my *Observations on various Authors, antient and modern*: and in truth I should often stand amazed at the numerous mistakes, misrepresentations, and improbable conjectures made by some and still received by other moderns concerning the words of antient authors, antient dates, and other articles, when so much better and more obvious solutions of apparent difficulties seem to stare us in the face; if it were not from my being sensible that favorite or current prepossessions do often so bias the judgment of men, that in many cases what is the most obvious as well as the most reasonable is the very last which is discovered or adopted; just as in mechanism, more complex and intricate methods of performing the same work are often invented and put in practice before the discovery of more simple and easy ones.

Now the *first* volume of these *Observations* concluded with establishing a *third* important



tant date, namely, that of the accession of Cyrus to the crown of Persia; and this we found to fall one year later than other modern chronologers have determined \*: the accession of Cyrus to the Persian kingdom fixes at the same time the *end* of the kingdom of Media; and we proposed to enquire next concerning the *beginning* of that empire: but this is of two kinds, either that when Arbaces and the Medes destroyed the Assyrian empire under Sardanapalus, and established a colony or kingdom of Medes at Nineveh, the capital of Assyria; or else when the Medes in the mother province of Media, growing tired of the tyranny of the new kingdom at Nineveh under the Median descendents of Arbaces, made a second defection, and threw off all subjection to the throne of Nineveh, setting up another kingdom of Medes at Ecbatana in Media itself, under Dejoces. In regard to this *second* kingdom the difference among chronologers concerning the date of its commencement amounts only to about ten years; those who prefer the ac-

(\*) Lud. Capellus is, I think, the only author who has hinted at the propriety of thus placing Cyrus's accession one year later than Petavius. "Cyri initium collocamus in anno *tertio* vel *secundo* Olympiadis 55." *Chron. Sacr.* p. 270.

count of Ctesias, thinking that they have reason on his authority to place it ten or twelve years later than those who adhere to Herodotus: in this however they are mistaken, and it will be shewn, that those two authors do in reality agree exactly together with respect to the date of this event; and also with Eusebius and all others (for they all place it in the 1st of the 18th olympiad) their apparent disagreements having only arisen from the error of 2 years made by most antients concerning the accession of Cyrus, together with another error of about 10 years made by the moderns themselves concerning the length given to the reign of Cambyfes by Ctesias, as briefly mentioned at p. 251 of Vol. I. Some evidence concerning the actual agreement of those antient authors relative to this date may be seen in my table at p. 216. v. 2. and it will be more particularly examined afterwards. At present our business is rather with the date of the commencement of the *first* kingdom of Medes, namely, that formed at Nineveh by Arbaces; for with respect to this date antient writers as well as the moderns do really differ, and even to the amount of an hundred years, more or less, as I have already noticed at page 344, v. 1: this then is a more difficult

difficult as well as more important subject of discussion; for it includes also the removal of a variety of uncertainties and incoherencies both in prophane and sacred history, particularly a determination of that long disputed question concerning the series of kings at Nineveh, mentioned in scripture, and which has been commonly and very properly called the second Assyrian empire \*. Now I have already, at the conclusion of my second volume, entered into an *enquiry preparatory* to the determination of this *question* concerning the accession of Arbaces; by examining the two relations of Ctesias and Herodotus, and shewing that there is in reality no material difference (as authors have pretended) discoverable between the two historic accounts of the establishment of

(\*) Vignoles indeed has suggested as if the knowledge of the duration of the Assyrian monarchy is of no service to illustrate the Jewish scriptures: "La durée de l'ancienne monarchie des Assyriens ne nous sert de rien pour l'éclaircissement de cette partie de l'histoire sainte." *Chron. Sacr.* 4. 4. But in fact it is of great importance; for without knowing the duration of that empire, we cannot ascertain the end of it; nor consequently, whether the first Assyrian kings mentioned in scripture flourished before the end of the first Assyrian kingdom, or after the beginning of the second.

these



these two kingdoms of Medes (as given by those two antients) the one under the conduct of Arbaces and the other under that of Dejoces : for both those accounts, besides their harmony in other circumstances, will allow of an agreement in the main point, viz. that the dissolution of the Assyrian empire by the Medes happened a *considerable* time before the exaltation of Dejoces to the crown of Media itself. Herodotus, however leaves the length of that interval entirely undetermined : Ctesias does indeed give some indirect though no *sufficient* assistance towards determining it ; because he has joined together the succession of reigns in *both* those kingdoms as if they had been but *one* (for they were both kingdoms of Medes, though in different countries) without marking under what king of the first kingdom the second kingdom began : if we reckon up the sum of that *whole* *succession* before Dejoces, we make the interval before Dejoces much longer than Eusebius, Syncellus, and Polyhistor, yet still not so long as Justin. For Justin makes Arbaces to commence his reign 163 years before the æra of Nabonassar ; Ctesias only 130 ; Polyhistor 120 ;\* but Eusebius and Syncellus

\* The sums here given are on supposition, that those authors reckoned the accession of Cyrus to have fallen

only about 70 : hence modern Chronologers have hitherto found themselves quite at a loss how to discover the true date among so many discordant ones; some have preferred one account and some another, and many do not agree with any one of those antient authors, but have formed new opinions of their own; yet in fact none of them have offered any sufficient reasons of preference for their own, but seem to have been guided rather by caprice or hazard in their several determinations, than by any motives of solidity. Usher and Prideaux have even gone so far as to reject all these dates of the antients, and have brought down the commencement of Arbaces so late as to be contemporary with the accession of Nabonassar; supposing the latter to be the same person as Belesis the coadjutor of Arbaces in subduing Sardanapalus. The date of this event in the present manuscripts of Paterculus, would place Arbaces still 28 years later than Usher; but critics have agreed, that the manuscripts have a corrupt reading there, although indeed none of them seem as yet to have proposed the right one in its place. It is evident on the 1st of 55th Olympiad, the 188th Chaldean year, as indeed most of the antients did suppose, though apparently not all of them, but some 2 years later according to the truth. *See Vol. 1. p. 345.*

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dent then, that there is still wanting some surer method and better evidence for determining the date of this event, the end of the Assyrian empire by the conquest of Arbaces; in order that we may not be eternally wandering about in the uncertainty of mere opinion only, and even in that be in a wavering state between the discordant accounts of the antients themselves as well as the moderns. Now I am not without hope of being able to accomplish this desirable object with as much certainty, as can be reasonably expected in regard to such an antient event, and one which may indeed with great propriety be considered as the earliest event in Asiatic history, which has any pretence to be accounted *probable*: for no one ought to expect demonstrative proofs, but only such as arrive at a high degree of probability, concerning events which hold the middle place between the fabulous and historic ages, and border as it were on both. The result of our investigations will be so much the more happy, as we shall find reason to think that the account of Eusebius and Syncellus, in placing Arbaces only 70 years before the æra of Nabonassar is both the nearest to truth, and is also confirmed by scripture; for it will follow hence, that king *Jarib*, who is twice mentioned in Ho-



sea as king of Assyria, was actually cotemporary with Arbaces, and therefore probably the same king, as will be proved otherwise. This is indeed altogether a new idea, never hinted at hitherto by any former writer, yet it is one, which the age wherein Hosea flourished and the dates of his prophecies will confirm; so that I must here again, as upon many other occasions already, beg leave not to be tyed down by any of the crude expositions of scriptural commentators, who have often so implicitly followed each other, that if the oldest ones have taken up any indigested notions, the later ones are almost sure of continuing them, and even indeed without examining the facts and evidence in question. But independent of the harmony which will hereby be pointed out between sacred and prophane history, I shall moreover shew by a minute examination, that even among prophane authors themselves, as discordant as they are with each other concerning the date of this antient event, yet they seem in some sort to disagree by rule and measure: so that we shall be able to discover many remarkable and harmonious co-incidencies in their different accounts, which seem to point out the causes of those disagreements, but in which Chronologers have hitherto seen nothing but inconsistency;

consistency; and the causes will help to extract the real truth and date out of their different relations. For just as different historians, although they may vary in many of the circumstances of some important event, yet all, nevertheless, generally contribute something to elucidate the true nature of it: so in the present case, if it shall appear that the disagreements among these authors have arisen not from mere errors at random, but that they vary on account of their having proceeded upon different principles and modes of computation; hence upon our tracing out the causes of their several varieties in computation, we shall at last find them all to contribute something towards leading us to the real and true dates.

Now Varro, we know, divided antient times into the *fabulous* and *historic* \*; the latter he conceived to begin with the Olympiads; whether he had any sufficient evidence to conceive the age of historic certainty to commence so early as with the Olympiads may admit of some doubt; but *possibly* he meant no more than only with respect to Greek affairs. This at least is *certainly* the meaning of Diodorus; for although he also pretends to begin historic certainty in the

\* Censorinus, c. 21.

transactions

transactions of the Greeks so early as with  
 the Olympiads, and calls nothing fabulous  
 after this in the history of Greece: yet the  
 case is quite otherwise with respect to his  
 account of the affairs of Asia; for he has in-  
 cluded his earliest relations concerning that  
 country within his first five books, which,  
 as he informs us, were destined to contain  
 only *fabulous* history; and he has there car-  
 ried down his relation of Asiatic events so late  
 as to the accession of Cyrus in the 55th  
 Olympiad. Here, therefore, I presume,  
 and not sooner, he conceived *historic certainty*  
 to commence in regard to Asiatic transac-  
 tions: but since almost all histories writ by  
 the ancients relative to the most early times of  
 the world have perished; I doubt whether  
 in the present state of things, we can have  
 any sufficient reason to attribute historic  
 certainty to any more early events with re-  
 spect to Asia, than those with which I set  
 out at first as foundation dates, the invasions  
 of Greece by Darius and Xerxes. Upon the  
 whole, therefore, it may be more advisable at  
 present to separate ancient prophane history  
 into the *fabulous*, the *probable*, and *historic*  
 ages. The *fabulous* age ends with the dis-  
 solution of the Assyrian empire by Arbaces,  
 and there also the *probable* commences both  
 in Greece and Asia, about 40 years before  
 the



the Olympiads, about 70 before the æra of Nabonassar and along with the first Assyrian kings mentioned in scripture, a full 100 years therefore before Hezekiah; which probable age continued down to the time of Cyrus, or rather Darius and Xerxes; where the age of *historic* certainty commences and not sooner. I having then in my *first* volume fixed accurately the dates of some principal events at the *commencement* of the *historic* age; the present continuation of that subject is destined to ascertain with more accuracy the date at which the *probable* age *began*, along with the accession of Arbaces, and a race of Medes as kings over Assyria, in consequence of their conquest of Sardanapalus and the first Assyrian empire: when we shall have thus fixed the two extreme boundaries of the *probable* age, namely, both its *commencement* under Arbaces, and its *end* under Cyrus or Darius, we shall be better able to fill in with accuracy all the intermediate parts; and indeed it has been this *probable* age, amounting to about 300 years, which has been the chief subject of dispute among Chronologers. Hereby we shall descend by degrees from the earliest to the later events in different nations, according to their right chronologic order; which by being the most natural, is also the most agreeable

agreeable order to readers, although we were forced at first to enter upon a retrograde course, in order that we might gain the firmer foundation, by proceeding from what was best known to what was more unknown, with respect to the dates of our earliest events in the *historic age*.

*General View of the Subject concerning the Commencement, Duration and End of the first Assyrian Empire, in the fabulous Age, together with the Means I shall employ to ascertain the Opinions of the Antients concerning these Points, and also the general Result of our Enquiries on the Subject.*

AS the *probable age* commences where the first Assyrian empire ends, we shall find it absolutely necessary toward ascertaining the commencement of the *probable age*, to enquire first concerning the *beginning* and *reputed duration* of that empire in the *fabulous age*. Its duration, as is well known, was by Ctesias and others reported to amount to about 1300 or 1400 years; but by Herodotus only to 520 years: the difference between these two accounts is so immense, that as the most ingenious Chronologers have exerted themselves in vain to reconcile those sums, it has been generally concluded  
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there must have been some great historic error in one or other of the two accounts, and indeed of such a magnitude, as could not well proceed from mere accidental mistake; but it rather betrays an intentional design, either in the relation of Ctesias to carry back too high the antiquity of the Assyrian empire, or else in that of Herodotus, to reduce it below its due bounds; yet on account the period really extending back either way so far into the *fabulous age*, no writers have hitherto been able to offer any satisfactory evidence, in which of the two computations it is, that the great error in question subsists. Hence some of the modern Chronologers have followed Herodotus, and others Ctesias; the chief foundation, however, for both of these different opinions, has been either a prejudice against the longer account of Ctesias, because of his romancing character, and also of that very circumstance of the enormous length of his period; or else, on the contrary, an ill-founded predilection for it, because it was by some persons erroneously conceived to agree best with the chronologic antiquities of the Jewish scriptures. Nevertheless, there are in reality some *historic* circumstances, which may be pointed out in the sum of Herodotus, though hitherto not



sufficiently displayed, and which form at least a *strong presumption* in its favor; and there are on the other hand some *astrologic* circumstances attending the sum in Ctesias, which equally form a *presumption* no way favorable to the veracity of that account, which the natives of Asia had communicated to him during his residence there, and he to the Greeks at his return home: for as his account was afterwards confirmed by Berosus, the Asiatic historian, it appears hence, that the romance was not of the invention of Ctesias; but derived by him from the natives of Asia themselves, and apparently founded on their own astrologic superstition.

The presumption unfavorable to the account of Ctesias arises from these facts, namely, that the genuine and original Assyrian period, as communicated by the Asiatics to that historian, consisted of the larger sum of one complete astronomic period of 1460 years, (as I shall shew to have been the real fact) and not of the lesser sum of 1300, to which it was afterwards reduced by the mutilations of the Greek Chronologers; and moreover, that one such astronomic period actually *ended* and a new one *began* (through mere accident) nearly about the very time, when the Assyrian  
 empire

empire was dissolved, at its conquest by Arbaces the Mede, as I have pointed out at p. 54 in *Append. Vol. III.* Now if these facts be true, it will immediately occur to readers to be very improbable, that the duration of the Assyrian empire should in reality have been precisely equal to an astronomic period, and to so large a one likewise as 1460 years; especially when it was also the first existing monarchy on record among mankind. It will appear much more probable, that the Asiatics attributed a complete period of 1460 years to their preceding Assyrian kingdom, for no other reason, than because it happened by accident to *end* nearly at the very time, which the Asiatic astronomers had a little before fixed on as the *beginning* of such an astronomic period of 1460; namely, at a synchronism of the sun and new moon with the day of the vernal equinox, in the 120th year before the epoch of Nabonassar: and that for this reason they afterwards conceived, or pretended, either through ignorance of the real duration of the past Assyrian kingdom, or through national vanity, or astrologic superstition, that this kingdom had *begun* at the *commencement* of the *preceding* astronomic period of 1460 years; because it thus accidentally *ended* nearly

about the time when the same period *ended* and a new one *began*, namely, during the 867th year before Christ; in which year on March 28 that astronomic period of 1460 *began*, which was current at the epoch of Nabonassar, and ever afterwards during Greek and Roman history. This necessarily follows from the certain fact, that the Chaldean and Egyptian new year fell on Feb. 26 at the epoch of Nabonassar, as appears from the dates in Ptolomy's astronomy: hence the Assyrians might be induced to extend back ostentatiously the duration of their past kingdom, so as to make it comprehend the whole past astronomic period of 1460 years. For the Oriental nations, even in much later and more intelligent ages, still adhered to the superstitious opinion suggested by astrology, that the rise and fall of empires were ruled by the revolutions, conjunctions, and oppositions of the planets: just as they, with equal error, pretended that there was a stated periodic return of similar seasons, winds, and tempests, after the revolution of an astronomic period of 1460 days, that is, of four years of 365 days each, without intercalation\*; and

\* Thus Pliny informs us, "Omnium quidem, si libeat observare minimos ambitus, redire easdem vices quadrennio exacto, Eudoxus putat, non ventorum



again, that the births, deaths, and fates of individuals, were in like manner determined by the conjunction or opposition of particular planets. So that the merely accidental *historic fact* of the Assyrian empire happening to be dissolved nearly about the very time when one such astronomic period ended and a new one began, would almost necessarily lead astrologic quackery to pretend, agreeably to the current superstitions

rum modo, verum etiam reliquarum tempestatum magna ex parte: est principium lustri ejus semper intercalari anno ortu caniculæ." *Lib. 2. c. 47.* The last words shew, that Eudoxus must have been speaking chiefly of the current opinion of the Egyptians, among whom he lived and wrote; but as the same retrograde year was in use with the Assyrians and Chaldeans as with the Egyptians, we need not doubt but they had similar superstitions and astrologic opinions connected with it. The only real fact in a revolution of 1461 retrograde years is, that it is equal to 1460 intercalated years, that is, two different civil modes of reckoning the annual courses of the sun, after the end of 1460 years coincide, and the two same modes begin together again: but the astrologers supposed erroneously, that all the heavenly bodies themselves were regulated by this period of 1460, so that if the sun, moon and planets were in conjunction at the beginning of such a period, they would be again in conjunction at the end of it. "Quantis conversionibus major ille, quem ferunt, perficeretur annus, qui quinque has stellas, lunam etiam ac solem locis suis originibusque restituit, qui 1461 annorum circuitu terminatur." *Astrolog. Jul. Firmici in Proem.*

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of the times, that this *historic* fact was connected with the concomitant *astronomic* one; and that such a great revolution in the kingdoms on earth was occasioned by a similar great revolution in the heavenly bodies: whence it was easy and natural for those possessed of such opinions to conclude, that since this Assyrian kingdom *ended* nearly at the conjunction of the sun and moon in the vernal equinox on the first day of their retrograde year, in a period of 1461, it consequently must have *begun* at a similar conjunction of those bodies on the same day 1461 years before. We have the greater reason moreover to presume, that this was the real cause and origin of that period of 1460 years assigned by the Asiatics to the Assyrian kingdom; because we have positive and convincing information preserved, that a similar effect was produced by a similar cause in a much later age. That any kingdom whatever, should in duration happen to be equal to a period founded on astronomical principles, but applied to astrologic purposes; and especially the very first kingdom recorded in history among mankind, equal to such a long one as 1460 years, is so very improbable, that these circumstances alone contain sufficient internal evidence of the truth of history having

having been here adulterated by some foreign motive for fable: accordingly the same author, Syncellus, who alone has preserved the memory of this original Asiatic sum assigned at first to the Assyrian kingdom, has also preserved an account of another similar period assigned to the Egyptian kingdom, founded on the same astrologic principle; but carried to still greater extravagance, though formed in a much later age by an Egyptian chronologer and historian, as the other by some Asiatic one. At p. 51, Syncellus gives us the chief contents of an old Egyptian Chronicle, which, according to his account, had been formerly current in Egypt; for he there presumes, that Manetho, who lived in Egypt not long after Alexander, was misled by it, but which could not have been composed above 20 years before Alexander. Now this pretended Chronicle extends back the duration of the Egyptian kingdom, so as to comprehend the whole of another and still more immense astronomic period of 36,525 years; after the expiration of which, the Egyptian astrologers conceived, that all the heavenly bodies, which were in conjunction at its commencement would be again in conjunction, and that then there would be a total restitution of all things, both in  
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the heavens and on the earth, to the same state as at the commencement of it. But what is this longer period, except only a complex multiple of the above Chaldean period of 1461 ? which, while it aims at being more accurate in some respects, in order to bring about similar conjunctions of the heavenly bodies, became less accurate in other respects than its original, the period of 1461, which it proposed to imitate and excel. For it was perceived by the astrologers, that although a year commencing at the vernal equinox, and intercalated once in four years, would after 1460 intercalated years, be equal to and coincide with a year not intercalated, but retrograde during 1461 years ; yet the commencement of these years would not then fall exactly, as before, at the time of the vernal equinox : because this had during such an interval moved backward in the zodiac, and would continue that retrograde motion until it had performed a complete revolution, which would not be in a *less* period, as they conceived, than 36,000 years ; that is, at the rate of 1 degree in every 100 years, called the platonic period, and adopted by Hipparchus and Ptolemy \*. Hence as they

\* It seems probable, that the Egyptians conceived, that

seemed desirous to improve, under pretence of accuracy upon the Chaldean and Assyrian period of 1461 retrograde years, they wished to multiply the latter, until it should be equal to that other period; they preferred therefore to multiply by 25, because in every 25 retrograde years, the new moons

that the period would be rather *more* than 36,000 than *less*, because we find among the Arabians, mention made of the period being 36,425; they probably had borrowed this from the Egyptians, and the latter had certainly adopted the still longer period of 36,525; but if they did not really think, that the period of retrogradation of the equinox amounted to *more* than 36,000, they made a voluntary error of 525 years, for no better reason, than in order to have the new moon in conjunction with the sun in the equinox, as well at the *end* of the period as at the *beginning*. See *Abul. Pharai Spec. Hist. Arab.* by Pocock, p. 145, and *Jackson, Vol. 2, p. 101*. Later observations, however, have proved, that the period would be *less* than 36,000 and not *more*, viz. 25,920 years only, which is at the rate of 1 degree in 72 years, i. e. 360 degrees multiplied by 72. Hence we find, that Jackson had no good reason to speak so favorably of this *magnus annus* of the Egyptians, when he says, "This shews with what skill and sagacity this grand period was formed and applied to the most useful purposes." *Vol. 2*. It has, however, at least one *historic* use, by proving, that the retrogradation of the equinoxes was known to the Egyptians before Hipparchus, in case Syncellus has rightly made this old Chronicle more antient than Manetho, who lived soon after Alexander.

VOL. III.

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would fall again upon the very same Egyptian day in their calendar of 365 days only: thus they arrived at the period of 36,525, which would have the moon in conjunction with the sun on the same Egyptian day, along with the equinox at the *beginning* of every such period; whereas, in a single period of 1461, this would not be the case, as 1461 will not divide by 25 without leaving a remainder. This was, however, only pains lost; for in order to be more accurate with respect to the new moon, they became less so in regard to the precise period of the revolution of the equinox; and in order that the new moon might not be eleven *days* distant from the beginning of the period in question, they increased this period to 525 *years* too much, and even more: but still it was founded upon the same astronomic principles as the Assyrian period, it being nothing more than a multiple of the period of 1461, yet under pretence of being more accurate in some points, it thus became less so in others; it was, however, applied with the very same astrologic view to comprehend the whole period of the existence of a kingdom in Egypt, as if the revolutions of kingdoms on earth were governed by the revolutions of



of the heavenly bodies \*. Now, how they contrived to extend the one on earth, in order to make it equal to their vast extension of the other in the heavens; and to what excess of extravagance they ran in this respect, as well as in the astronomic period itself, let us next inquire. The Assyrian kingdom was ended by the conquest of it under Arbaces the Mede, either about 70 years before Nabonassar, or at most about 100, that is, either 50 or 20 years *later* than the 120th year before Nabonassar, in which the Chaldean period of 1460, then current, *began*, viz. on March 28†. There was then some plausible pretence for the Assyrians to affirm afterwards, that their preceding famous kingdom *ended* at or nearly about the *end* of one of their astronomic periods, and the *beginning* of an-

\* In case the period of 36,000 was called the *platonian* year, on account of its being known to and mentioned by Plato, its antiquity would hereby be carried back long before the age of Alexander. But I cannot find any account of the origin of this appellation *platonian* year; it is possible, that it may have been called so only from its being celebrated by the later platonists, as they are called, who lived in Christian times during the third century, and who in many cases attributed to Plato opinions never held by, or known to him.

† See p. 55, &c. in Appendix to Vol. 3.

other ; but the case was very different with the Egyptian kingdom 500 years afterwards: for the old Chronicle above-mentioned places the *end* of this at its final subjection to the Persians, after the death of the last Egyptian king Nectanebo, which was in the 20th year of Ochus king of Persia, about 340 years before Christ; therefore not less than 480 years *before* the *end* of the then current astronomic period of 36,525, according as the Egyptians accounted it to *end*; for this could not be until the 139th year of Christ. Because they pretended, that their current great astronomic period, or *magnus annus*, after which was to be a total renovation of all things, had *begun* at first, and would consequently *end* again, whenever the new year's day of their retrograde year should, after one complete revolution, that is, after 25 periods of 1461 years, fall at that season and day of their year, on which the dog-star would rise in Egypt heliacally, (that is, along with the sun, yet so as to emerge from his rays, and be visible before sun rise). Now this happened in Egypt in the 139th year after Christ; for then the Egyptian new year's day fell, for the 4th time, upon the 20th of July, and on the same day the dog-star rose heliacally,

heliacal, as Censorinus has informed us, and it is confirmed by the moderns \*. If then to the above 340 we add the 139 after Christ, they amount to 479 years, that the Egyptian kingdom ended *before the end* of the grand period of 36,525, then current. This shews how little scrupulous the antients were about any kingdom in question, ending *precisely* with the end of their current *magnus annus*; for if it were within 500 years sooner or later, they would, we see, account such deviation to be of no importance, so as to form any objection to the propriety of their saying, that the Egyptian kingdom was comprized within a whole period of 36,525 years; nor consequently to the Assyrian kingdom being comprehended within a whole Chaldean period of 1460, notwithstanding that it continued 20 or 50 years *later* than the *end* of that period in the 120th year before Nabonassar. Still farther, although this grand period would *end* on July 20, in 139, yet, as

\* "Ab hinc [i. e. ab ann. 239, Jul. 20] retro annos centum in consulatu Antonini 2 & Bruttii, [ann. 139] initium anni Nabon. fuit Jul. 20, quo tempore solet canicula in Ægypto facere exortum; quare scire licet anni illius magni (1461) *nunc* agi vertentem annum centesimum [i. e. a Jul. 20, 139, usque ad Jul. 20, in 239.]" *Censorin. c. 21.*—*vid. Auctarium Petavii, lib. 5. c. 6. in Uranologio.*



Petavius there shews, it would not have *begun* on that day 36,525 years before, for the dog-star would not then have risen heliacally on that day in Egypt, unless by some great accident. Hence we find again, how inaccurate was this theoretic prolusion of astrology among the Egyptians, on which Jackson has so idly bestowed commendation; and hence we may further conclude, that no greater accuracy was consulted in the *historic* contents of these periods than in the *astronomic* contrivance of them, but that the former was purposely and fabulously invented, so as to suit the latter: and this will appear more fully if we examine the *historic* contents of the Egyptian period of 36,525, which happen to be better known than those of the Assyrian period of 1460; but from the romancing nature of which former, we may form a competent judgment concerning the latter being nearly of the same nature\*. Let us observe then, how the compiler of the old Egyptian

\* This, indeed, even Jackson himself seems to allow, when he observes, "That the first portion of the old Egyptian Chronicle, from the reigns of the gods to the *Cynic* cycle was merely fictitious, to make the Egyptian nation *as old* as Berofus had made the Chaldean—but the rest from the *Cynic* cycle to the end of the Egyptian empire, is founded on their antient records and monuments." *Vol.* 2. 98.

Chronicle contrived to fill up the first and altogether *fabulous* portion of the period in question, containing 34,644 years; it was with imaginary reigns of gods and demigods; for the only portion of it, which contains any thing of a real *historic* kind, is the last portion of 84 reigns of mortal kings, amounting together to 1881 years: it is possible, that some archives and traditions might have been delivered down to the age of the compiler, from which he formed this last portion; as on an average, it would be about 22 years to each reign, which is consonant enough to the average length of reigns in modern times. But these 1881 form but a small part of the whole period; and all the first, which is the greater and more unknown portion, he filled with fabulous reigns of all the gods and demigods best known in Egypt; to some of whom he did not scruple to give several hundred years, and to the others as many thousand years of reign, as were necessary to fill up the whole period. May we not then reasonably conclude, that the case had been nearly similar before in the Assyrian period of 1460; between which and the old Egyptian Chronicle we have found so many articles of resemblance, and which former one, indeed, seems to have been

been the pattern intended to be imitated by the compiler of this Egyptian Chronicle in a later age, yet not very long after Berofus had in his *Asiatic history* made known the nature of the Chaldean period of 1460, together with the ostentatious pretences of the Assyrians, to a duration of their kingdom during the whole of such a period. It was then through such views as these, and by such means, that the 520 years of real Assyrian empire, as collected by Herodotus and the Greeks from the number of intervening generations, seem to have been extended by the astrologic superstition and national vanity of the Asiatics, so as to fill up one whole astronomic period of 1460. But these favorite whimsies of astronomic periods, which were to produce a total renovation of all things, together with the *astrologic* opinions attached to them, are as *unfavorable* to the veracity of the Chronology pretended to be contained in them, as on the contrary we shall find the *historic* facts mentioned by Herodotus concerning the Assyrian kingdom, such as that of Belus first king of it being grandson to Hercules, and Agron first king of Lydia grandson to Belus, to be *favorable* to his computation of only 520 years in all, from Belus to the end of that kingdom: and at

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the same time they are in such consistency with the rest of the Greek history and chronology, that it would have been impossible for Herodotus to have assigned a larger sum without subverting all the principles of computation received among the Greeks in the age of that historian.

For Herodotus informs us in another passage of the same first book, " that Belus was reputed to be the son of Alcæus, the son of Hercules \*:" but it is well known, with respect to Hercules, that the genealogies of the several royal families in different kingdoms of Greece, descended from him according to tradition, agree in fixing with some degree of certainty, *the age* in which this personage was reputed to have flourished; namely, during the course of that century, which immediately preceded the Trojan war. Moreover, he was in his youth when he became a companion of those who engaged in the Argonautic expedition, which, as is agreed by all, was about 80 years before the siege of Troy; at which siege, still farther, it is proved by Homer, that Tlepolemus, the youngest of the sons of Hercules was present, as were

\* " Heraclidarum primus Sardium rex extitit Agron, Nini filius Beli nepos, Alcæi pronepos, qui fuit Herculis filius ex ancilla Jardani." *Lib. 1. c. 1.*

likewise some of his grandsons by elder sons. Since these facts then fix the *age* of Hercules to be during the century before Troy, so according to the Chronology of Herodotus himself, the capture of Troy was something *more* than 508 years before the first Olympiad; this was about 14 years sooner than the computation in the Life of Homer, erroneously ascribed to Herodotus, which amounts to 494 years only before the Olympiads, and both of them are a few years *more* than Thucydides computed: but in this at least they all agree, that the authors of this *middle age* made nearly a whole century more of this interval than the historians afterwards in the age of Alexander, viz. Ephorus, Timæus and Eratosthenes, who reduced it to about 407 years only\*. If then the end

\* Ad me sunt anni ferme nongenti ab Hercule; a Pane vero minus annorum est quam a bello Trojano, octingenti fere usque *ad me*." [*εξ εμε*] *Lib. 2.* M. Larcher, the late translator of Herodotus into French, has strained the obvious sense of these words, so as to make them suit his own purpose, (as indeed others have done before) of fixing the capture of Troy at 494 years before the Olympiads in 1270 before Christ; this, indeed, is the real date in the Life of Homer, erroneously ascribed (as he allows) to Herodotus, but not as he pretends also the date here in Herodotus, and in Thucydides and other antients. It would take up too much time

of the 520 years of the Assyrian kingdom was about 70 years before the epoch of Nabonassar, these two sums added together

time to examine the inaccuracy of all his computations, and I shall only observe, that the present one of Herodotus amounts to *more* than 494, as does that of Thucydides to *less*, although indeed they do not differ many years: yet, they differ enough to prove, that Larcher's favorite date of 494 years before the Olympiads, that is 1270 before Christ, has no better claim to accuracy than any other, sooner or later, for the year of the capture of Troy; and moreover, that the antients differed so much in their computations of this event, that no two of them have agreed together, consequently that no argument can be drawn, as Larcher pretends, from any general agreement in favor of 494, or of any other particular year. Now, he allows, that Herodotus was born four years before Xerxes passed the Hellespont at the end of the 74th Ol. in 480 before Chr. Those 73 Olympiads then, equal to 292 years, must be added to 508, or *more*, to make up the 800 from the *birth* of Pan to the *birth* of Herodotus. Bouhier indeed, Larcher and others dispute, whether *ad me* [εἰς ἐμὴν] means to the *birth* of Herodotus, or the time of *writing* his history, or of *reciting* it at the Olympic games when he was 40 years old: but I cannot see the least room for any other sense than that of his *birth*; for he fixes Hercules at 100 years sooner than the siege of Troy, therefore he certainly reckoned *from* the *birth* of Hercules. So in like manner he reckoned from the *birth* of Bacchus and of Pan, why then should he not reckon *to* his own *birth* only, and not to any later event of his life? His words are, "A Dionysō, qui ex Semele *genitus* fertur *ad me* sunt anni 600; ab Hercule Alcmenæ *filio* 900; a Pane qui



amount to 590, as the date of the accession of Belus before Nabonassar; consequently, as the 1st Olympiad in 776 before Chr.

ex Penelope et Mercurio *genitus* dicitur—800 *ad me.*" In all these three computations he plainly reckons from their *birth*, more especially in regard to Pan, for he adds, "de Pane ne habent quidem quod dicant, ubinam *a partu* sit educatus." So that as nothing more was known of Pan than his *birth*, he must reckon *from* that, why then not reckon to his own *birth*? Accordingly, Larcher himself in one place supposes this: "If we add 900 to 484 before Christ, the date of the *birth* of Herodotus, we have the date of the *birth* of Hercules." P. 379. Yet, in contradiction to this, in another place he supposes the 800 to be reckoned from the *birth* of Pan to the *age* at which Herodotus travelled in Egypt, and might *write* these words in his 2d Book concerning Egypt. "He was born 484 years bef. Chr: he travelled into Egypt at 460; and Pan being born 800 before *this epoch*, this was 1260 before Chr. but his birth being 10 years after Troy, these together make 1270." P. 392. By *this epoch* he refers, as his dates prove, to his *time* of travelling into Egypt when he was 24 years old: but this is mere unwarranted supposition, contrived on purpose to make the 800 years *begin* 24 years *later* than they would otherwise do, if reckoned down no later than to the *birth* of Herodotus in 484, which would fix their commencement in 508 before the Olymp. exclusive of the uncertain 10 years mentioned above. By M. Larcher's thus *supposing* what he pleases, he *brings down* the computation of Herodotus for the capture of Troy to 1270 before Christ instead of 14 years earlier; and by a similar contrivance he has *raised up* that of Thucydides to 1270, in order to make them countenance and confirm each other.

Concerning

preceded the epoch of Nabonassar in 747 by about 29 years, Belus must have preceded the 1st Olympiad by 561 years, that is 53 years before the date of the capture of Troy according to Herodotus, in 508th year before the Olympiads. Now Alcæus, the reputed father of Belus, was the eldest of the living sons of Hercules, having been born of a servant maid of Jardanus, king of Lydia, immediately after the Argonautic expedition; which being 80 years before Troy, there were consequently only about 27 years from the birth of Alcæus

Concerning this latter I shall only observe, that although the extract from Conon in Photius. (*cod.* 186, sect. 36) says, that Melos was founded in the 3d generation, yet sect. 46 in the same extract, says, it was *at the same time* that Nelius led the Ionians into Asia, which Petavius from the antients fixes at only 130 years after the capture of Troy: but which Larcher augments to 155, by reckoning the *two whole generations* above-mentioned at the greatest possible sum of 67 years, and eight years more, because Conon says *in the third* generation, so that he includes eight years of the 3d generation; and in all 75, after the Heraclidæ entered Peloponnesus, which, added to the 80 between Troy and the Heraclidæ make up 155, &c. between Troy and the foundation of Melos. Thus Larcher makes Thucydides and Conon together *raise up* the capture of Troy 25 years earlier than their dates would state it, if there were only 130 years between Troy and the Ionic emigration. (*P.* 396, *tom.* 7.)

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to the foundation of the Assyrian kingdom by Belus his son, 53 years before the capture of Troy. This is certainly *too short* an interval on *an average*, of short and long lives, as Belus must have been then arrived at *maturity*; and there ought to have intervened, at *an average*, one generation and a half at least, between the *birth* of the father and the *maturity* of the son, that is 30 years and 15, or in all 45 years at least, if not 50 or 60; but 27 is within the bounds of *possibility*, and whatever is possible, does sometimes happen, consequently it might have happened in this particular instance, so far as we know, namely, that Belus was begot by Alcæus, when the latter was only 14; and moreover, that he acquired the kingdom of Assyria at the age of 14 through the interest of Hercules and his friends in the neighbouring kingdom of Lydia, where Hercules had resided, and his fame was well known. This then is the only defect which subsists in the dates of these *historic* facts of Herodotus; but even this will be removed, if we recollect, that the accounts of Eusebius and Syncellus place the *end* of the Assyrian kingdom only 50 years, and even less before Nabonassar instead of 70; which variation, by fixing the *commencement* of the Assyrian kingdom



kingdom 20 years or more *later* than before, would extend the preceding interval between it and the prior birth of Alcæus to 47 years at least, and thus come near to an *average* interval. Still farther, we have reason to believe, that the computations of Hecateus and others, the earliest Greek Chronologers before the *middle age* of Herodotus, placed the siege of Troy at more than 508 years before the Olympiads, if not nearly at 560 years; they likewise fixed the Argonautic expedition at 100 instead of 80 years before Troy; either of which variations would add other 20 years or more to the above interval, and thus possibly extend it to 50 or 60 years. In the last place, besides the foregoing *three* methods of vindicating these dates and sums in Herodotus from error and incoherency, it should be remembered likewise, that although Herodotus adopted the reduced computation current in his own age of *about* 500 years only, from Troy to the Olympiads, yet he might have borrowed those other facts and computations from Hecateus and other historians who preceded him, whom he sometimes quotes; but without making any proportional reduction in them, and thereby might have produced the above-mentioned small and only improbability in his

his accounts, by making rather too *short* an interval between the birth of Alcæus and the commencement of the Assyrian kingdom by his son Belus, in case there be in reality too short an interval; for I have shewn, that it might be an apparent one only, caused by ourselves in placing the *end* of the Assyrian kingdom *too soon* before Nabonassar, together with our possible ignorance of some other material circumstances. Thus then, one way or other the 520 years assigned by Herodotus to the duration of the Assyrian kingdom, is very sufficiently reconcileable to his other account, that Belus was reputed to have been the grandson of Hercules; and both those facts agree with the chronologic antiquities of the Greeks, so far at least as these agree with each other, and with probability: for if the grandsons by some of the younger sons of Hercules, and also the youngest of his sons were present at the siege of Troy, there can be no improbability that a grandson by the eldest of his sons should have arrived at maturity 50 years before that event; since Hercules was a youth at the birth of his eldest son Alcæus, and lived nearly 50 years afterwards\*. We

\* It may, however, be still objected to this argument in favor of the veracity of Herodotus,  
con-

ought not to wonder therefore, if some apparent improbability in this account (in case there really be any) should have arisen

concerning the duration of the Assyrian kingdom, by its being consistent with his computation of the *age* in which Hercules lived, the reputed grandfather of Belus, the Assyrian founder, it may (I say) be objected, that the tales concerning Hercules are just as dubious and fabulous as the duration of the Assyrian empire. Now this, I allow, and possibly that even the existence of any such person as Hercules may be fabulous likewise; but this forms no objection against the above chain of reasoning, because nevertheless *the age* in which this fabulous personage is reputed to have flourished, may have been rightly assigned to the century before the capture of Troy; and it is only *the age* in which he did or was reported to have lived, which is of any moment in our reasoning, whether his existence was altogether fabulous or not. This *age* is fixed by the genealogies of the several royal families in Greece, believed to be descended from him; which genealogies were real and true, although the current report of the *head* of each family having been a *son* of Hercules, should have been ever so false. These genealogies, when traced back to the age of Hercules, together with their several intermarriages, all correspond in such wonderful harmony, as could have happened only by their being founded in truth, and faithfully preserved. Now these carry back those *heads* of families, called *sons* of Hercules, into the middle of the century before Troy; and if those currently reputed *sons* of Hercules had been called by the vanity of their descendents *sons of the moon*, the veracity of their several descents and genealogies would not be hereby impeached, nor yet the *age* in which such *heads* of families lived be any way what-



from the same cause which has produced so many other discordancies in Greek Chronology, and some even to the amount of a whole century; that is, that when the later Greek Chronologers shortened the interval between Troy and the Olympiads to the amount of 100 or 150 years, they did not make a proportional reduction in the dates of all other antient events connected therewith, but incoherently intermixed the Chronology of the earliest Greeks with those of later ages. Hence, if in the present case it could be proved, that the *end* of the Assyrian kingdom was even 100 years or more, instead of 70 or 50 before Nabonassar, (as some antients have affirmed) yet as there is equally good evidence, that the earliest Greek historians placed the siege of Troy at not much less than 560 years before the Olympiads, a reasonable reduction therefore ought to have been made

ever altered: it would be only the same as to say, that we have traced back our genealogies into such a *dark age*, that we can trace them back no farther, therefore we call those first *heads* of each family *children of the sun and moon*, or *autochthones*, as if they had sprung out of the earth of those regions where they lived. In fact, Hercules is affirmed by some of the antients, and believed by some modern critics to be only another name for *the sun*, as also are Belus, Bel and Baal.

in

in other dates, when the historians of the age of Herodotus reduced that interval to 500 years, and those in the later age of Alexander to 407 only before the first Olympiad, in 776 before Christ\*. The

\* Another chronologic computation in Herodotus, either confirms, or at least is sufficiently in harmony with his duration of 520 years for the Assyrian kingdom, to prove that the latter was not a hasty random assertion, but the result of systematic enquiries into antiquity, conformably to the estimation of about 500 years between Troy and the Olympiads. For he informs us, "That Agron was grandson to Belus, and founded or acquired the Lydian kingdom 505 years before Gyges." (lib. i.) Now according to my own system at p. 216, vol. ii, Gyges would have acceded to the kingdom of Lydia about seven years *earlier* than in other systems, viz. about 722 years before Christ, therefore Agron began 1227 years before Christ; from which, deducting 776, the date of the 1st Olympiad, there remain 451 years from Agron's accession to the Olympiads, which was therefore 57 years *after* Troy, at the calculation of 508 years interval. So that there were 110 from the accession of Belus, 53 years *before* Troy, to the accession of Agron his grandson 57 *after* Troy. Now as a former interval was *too short*, so this is indeed rather *too long* between the maturity of a grandfather and a grandson; yet not so much, but that both computations appear to be coherent parts of one and the same chronologic system, especially if we consider, that we are ignorant whether Agron might not have been *past his maturity* and in old age before he acquired Lydia, which fact would take off possibly 20 years of the above interval. Again, we are not certain, whether the

above *historic circumstances* therefore contained in the account of Herodotus, by being thus in harmony with the duration of 520 years assigned by him to the Assyrian kingdom, and both of them in harmony with the chronologic accounts of other Greek authors, concerning the time at which the Assyrian kingdom *ended*, make as much in favor of his *shorter sum* as the *astrologic circumstances* attending the account of Ctesias and the *longer period* of 1460 make against the probable veracity of it. Having thus presented a *general statement* of what appears to me agreeable to truth concerning this subject, let us next examine and verify any *particular portions* of the above statement, which may be thought to

Assyrian kingdom might not have ended *later*, i. e. 50 years only instead of 70 before Nabonassar, whereby Belus would have acceded 20 years later than 53 before Troy; this would take off other 20 years; and these 40 years deducted from 110, reduce the interval to 70 years, which does not exceed probability in *particular cases* for two generations, although it may be still too large a sum for a *general average*: and moreover, if there be any truth in the profane accounts, both Belus and Ninus had long reigns assigned to them, the former of 50 or 60 years, the latter of 45 or more; nay, even although there should be no truth in those accounts, yet such traditions may have influenced Herodotus in his computations to make them long.

stand



stand in need of illustration or confirmation; and thus we shall be best able to discover and point out the numerous errors both in facts and reasoning into which other writers have fallen, and which have prevented them from coming to the same conclusions with myself in the above *general view* of the subject.

The errors and false opinions of modern Chronologers, concerning the information transmitted to us from antiquity relative to the Assyrian empire, have in truth been so numerous and contradictory, that I scarcely know where to begin, in order to point them out clearly to my readers, together with the causes which have led to them. The general method of writers has been to refute and rectify first of all the mistakes of others, and after this to exhibit their own opinions; but this method is attended with the following disadvantages, namely, that the right and true view of the subject is kept too long out of sight, by its being thus postponed until after a long series of altercations concerning errors: and moreover, it is less easy for readers to judge properly of the solidity of my objections to the erroneous opinions of others, until after they have themselves gained a clear view of the true state of the subject; so that  
they

they are hence too often involved in confusion, by being kept in suspense concerning the truth, and by being shewn only what is not true. I have therefore inverted the above method, and have exhibited first a brief and *general view* of what I conceive to have been the real truth, together with *some* evidence in support of it; reserving *the rest* of my evidence and arguments for a more minute examination of the several *particular* portions of the subject, which may be either liable to some doubt, or have actually been represented by other writers in a view very different and often directly opposite to that in which they are exhibited in my own *general statement* above. By this method I shall have an opportunity, after having pointed out the *truth*, to shew also the *errors* which have been committed concerning it, either occasionally in the text, or in the notes subjoined to it; by which means readers will more obviously and clearly discern the latter, after their minds have been properly prepared and illuminated with a full view of the former, and we shall hereby also proceed in a more methodical manner in the discussion of both \*. I cannot, however, undertake to

\* As one example of this it may be observed,  
that

refute all the errors of all the numerous Chronologers; this would take up folio after folio, and may be safely left to the

that Vignoles was so prejudiced against the short duration of 520 years for the Assyrian kingdom in Herodotus, as even to affirm, that he was totally ignorant of Assyrian history. But what was one of his *chief* arguments against it? It was, that it made a grandson exist before his grandfather. "According to Herodotus, author of the paradox in question, and Usher, who adopted his account, Ninus, the great grandson of Hercules, founded the Assyrian kingdom 83 years before Troy, when Hercules was only six or seven years old; and Belus, the grandson of Hercules, reigned in Assyria 50 years before his grandfather was born: Is not Herodotus a good guide in the History of Assyria?" *vol. 2, p. 176.* But the whole foundation for this accusation is, that Vignoles here jumbles together incoherently the computations of Herodotus, who allowed 508 years for the interval between Troy and the Olympiads, with the computation of Usher, who adopted the reduced sum of 407 years only; this latter consequently made Hercules born 100 years *later* than Herodotus did, and thus later than his grandson Belus. The accusation of Vignoles, therefore, may be a good one against Usher, for adopting one computation from Herodotus and another from Eratosthenes, and then incoherently mixing them together in the same system; but it forms no objection to Herodotus himself, whose own computations are in harmony, as I have shewn in the above *general view*. When Usher brought down the siege of Troy and the age of Hercules 100 years later than Herodotus did, he ought to have also brought down all the other computations of Herodotus 100 years *later* at the same time. So that the accusation of Vignoles  
is



sagacity of readers themselves, after I have pointed out the true state of any particular article in question, and the foundations of the *chief* errors concerning it, into which several authors have fallen; and consequently have shewn the way, whereby all those other errors likewise may be refuted into which Chronologers have been misled, and in which they have too often blindly followed each other \*.

is only an argument against the patch-work of modern systems, in taking one date from one antient and another from some other antient, thus forming an incoherent whole.

\* It were to be wished that they had equally followed each other in rejecting unsolid conjectures and accusations, the inanity of which had been sufficiently exposed by some of their predecessors; thus M. Freret had in 1725 obviated this very accusation, which Vignoles renewed in 1737. "All the difficulties suggested against the sum of 520 will disappear, if we only explain Herodotus by himself, and examine the Chronology which he has adopted in his history; we ought not to subject him to the chronologic hypotheses of antient authors who lived 200 years after him, who have allowed 100 years less of interval between the siege of Troy and the Olympiads than Herodotus did himself." *Mem. de Litter. tom. 7. Chronologic de Lydie par M. Freret.* Notwithstanding which, M. Bouhier, (in 1746) did not perceive, that the censure by Vignoles did not affect Herodotus, but only Usher and other Chronologers; for instead of removing it in the manner of Freret and myself, he allows its force, and attempts to refute it only upon the foundation, that the

*The Conjectures of the Moderns, whereby they would reconcile the Accounts of Herodotus and Ctesias, shewn to be Unsolid and Untenable.*

THE first error of modern Chronologers, which it may be expedient to mention, is an attempt to reconcile the sums of 520 years in Herodotus with the longer sums of Ctesias, by supposing an error of the copyists to have crept into the text of Herodotus, through the omission of an ( $\omega$ ), the Greek numeral for 1000, whereby instead of 520 they would read 1520. This was first started, I believe, by Jac. Cappellus a century ago, and yet

the Belus in question was not the king of Assyria. (*Diff. sur Herod. p. 17.*) M. Larcher, however, in his notes and chronology annexed to his late French translation of Herodotus in 1786, rightly refers to and approves these words of Freret; but nevertheless, even he has made no use of them in justifying the 520 years of the Assyrian kingdom in Herodotus, as being both the true sum of the *whole* of that kingdom, and also in consistency with his other account of Belus being the grandson of Hercules. My own foregoing vindication therefore of the harmony between these different chronologic and historic accounts of Herodotus and other Greek authors, is the first which has been hitherto published, although so many persons have writ on the subject.

this wild fancy has found admirers almost down to the present times, as if they were never willing to relinquish the unreasonable opinions of their predecessors; and this it is which causes so much confusion in Chronology, the chief part of our time and paper being occupied in removing the rubbish, with which others have overwhelmed the subject\*. Marsham, about 1672, was at first caught by this spirit of conjectural emendation, but instead of an  $\alpha$  being *omitted*, he preferred to suppose that it had been *altered* into a  $\kappa$ , and thus  $\phi\kappa$  (520) made out of  $\phi\alpha$  (1500) read backwards, (p. 489, al. 522). Soon after, however, he retracted this desperate conjecture; but Simson and the very learned H. Vossius nevertheless adopted it, who made also another addition, viz. that the same correction ought to be made in the text of Diodorus, where the sum of Herodotus is quoted; and other still later authors have testified an approbation of these conjectures†. But no MSS. of Herodotus or Diodorus have, I believe, been

\* "Herodotus habet annos 520; sed omnino omissa videtur millenarii nota ( $\alpha$ ) pro  $\phi\kappa$  (520) legere debemus  $\alpha\phi\kappa$ , 1520." *J. Cappell. Hist. Sacra, &c. Sedan 1613, p. 45.*

† "Probo conjecturam Marshami—nec dubito quin etiam apud Diodorum  $\alpha$  exciderit, aliter enim non



ever found, in which those sums were expressed by numeric letters, and not in words at length; moreover, as the sum in Diodorus is only 500, there never could have been in this author a numeral  $\alpha$  altered into  $\kappa$ ; but  $\alpha$  must here have been omitted if there be any such error at all: and even if it had been thus, yet 1520 would be almost as much too great a sum instead of 1300, as 520 is too little. But as it can never be reasonably supposed, that all the MSS. of Herodotus had been thus corrupted in the age of Diodorus, his round sum of 500 is alone a sufficient refutation of all these fanciful conjectures. (Vide my vol. 2, p. 389). Still farther, it is certain that Appian adopted the very same sum of 500 years for the Assyrian kingdom, and this he probably must have borrowed from Herodotus, all other historians having adopted the larger sums of 1300 and 1400 from Ctesias\*. Agreeably to this we

non absque reprehensione istum Herodoti locum esset prætervectus." *Vossius de 70 Interpr.* 373. (1661).

\* "Assyriorum, Medorum et Persarum temporum simul congesta usque ad Alexandrum non annos quidem nongentos attingunt." *App. Præf.* Now the Medes, after *Dejoces* ruled about 150 years, the Persians from *Cyrus* to *Alexander* about 230, in all 380, which deducted from 900, leaves only 520 for the Assyrian kingdom.

find Porphyry, an able Chronologer, placing Semiramis about the time of the Trojan war, according to *a written tradition* \*: and the currency of such a tradition is confirmed by a Greek scholiast quoted by Casaubon, [ad Julii Cæsaris vit. in Suetonius, c. 22.] “Kaystrus was the son of Penthesilea the Amason, who in Ascalon married Desceto, [Derceto] and of her begat Semiramis, who built the walls about Babylon.” Penthesilæa is mentioned by Homer as being present in the Trojan war, her grand-daughter would flourish therefore about the same time after the capture of Troy as Porphyry and Herodotus suppose; for Belus, according to the Chronology of Herodotus, began 53 years before Troy, and he was succeeded by Ninus, and his wife Semiramis. Thus then the reading of 520 years in the present text of Herodotus, is confirmed by Diodorus and Appian; and this Chronology is either confirmed by similar current traditions, or else these traditions were derived from his Chronology, and thus confirm the numerals in his present text. Nevertheless, Strauchius in 1667

\* “Sanchoniathon vixit sub Semiramide Assyriorum regina, quæ ante Trojana tempora aut circa illa fuisse traditur.” [αναγεγραπται] Euseb. Præp. Evang. lib. 1.

did not seem adverse to these conjectures of Capellus and Marsham, although indeed at last he expresses some *doubt* about them; but Vignoles in 1737 was, I believe, the first who absolutely reprobated them; this, however, was only in order to substitute another opinion of his own as little defensible, namely, that Herodotus was totally unacquainted with Assyrian history, so that his sum of 520 years arose from his own ignorance, not from errors of the copyists \*. Whether an author, who is capable of such errors as are pointed out

\* "To say my own opinion freely, I think that Herodotus had little or no knowledge of Assyrian history, he says but little of it; he enters into no particulars, or is not exact in what he does say; he did not even know who was the founder of it." P. 175. All these accusations are sufficiently answered by this circumstance, that he reserved these things for his professed *History of Assyria*, (as Bouhier has rightly observed) which he informs us he should publish; and that he did publish it there is reason to believe, as Aristotle quotes a passage from Herodotus, which must probably have been taken from that history. This accounts for his brevity in his present work concerning Assyrian affairs: in fact it is Vignoles himself who is inaccurate in this very account as well as prejudiced; for he makes Herodotus say, "that the Medes were subject to the Persians 128 years;" but what Herodotus really says is, that the Medes ruled Asia 128 years. [Cet historien ajoute, que les Medes ont été assujettis aux Perses durant 128 ans]. P. 177. It is such  
hasty



in the subjoined note, and such false reasoning as I have pointed out before, has any right to advance this assertion readers themselves will judge; but I have mentioned these particulars chiefly, in order to shew, that former chronologers have been influenced more by hasty prejudices against, or favorite predilections for the relations either of Herodotus or Ctesias, than by any due comparison between them founded on just criticism and solid evidence. Even those writers, however, who have totally rejected all such untenable conjectures and accusations, have still only substituted another erroneous notion of their own, which is indeed something more specious though not more solid: this is not by altering the text of Herodotus, but by giving a different interpretation to one part of his words; and this was, I believe, first suggested by Cumberland, who, when Herodotus says "that the Assyrians had ruled over the upper part of Asia 520 years before the Medes revolted," [vide p. 389], confines those 520 years of government strictly to *Upper Asia and the Medes only*; thus allowing, that the Assyrians had ruled over the

hasty accusations which cause so much confusion in Chronology, and make us to lose so much time and paper.

single

single kingdom of Assyria *long before* those 520 years commenced, at which time they first begun to extend their conquests to *Upper Asia* \*. If this sense could be confirmed as the real meaning of Herodotus, it might indeed reconcile his account with that of Ctesias: M. Freret had put the very same construction as Cumberland on these words of Herodotus about the very same year 1724, apparently without any communication with each other †; and this sense has been greedily seized on ever

\* " By Herodotus affirming that the Assyrian dominion had stood so long as 520 years *over Upper Asia*, he does not deny that it had stood *no longer over another part of Asia*, and it is likely, that he did not know when it *began*; he does not pretend to know where Ninus or Nimrod lived, nor from whom the city of Niniveh took its name. It is not certain how far *Upper Asia* did extend, and it is probable that some considerable part thereof might be first added to the *older empire* about 520 years before the Medes revolted." P. 249. [1724].

† " It is very probable, that Herodotus placed the *foundation* of the Assyrian kingdom *before* the commencement of their empire and dominion *over Upper Asia*: we see that he does the same by the Medes, for he reckons only 128 years for their dominion *over Upper Asia*, although the *total duration* of their kingdom was 150." Tom. 6. *Essai sur l'Histoire des Assyriens*. But these deficient 22 years were not at the *beginning* but at the *middle* of their empire, namely, under Cyaxares, while the Scythians over-ran Media, and are expressly excepted. L. 1.

since

since by all those Chronologers, who have been possessed with the spirit of reconciling these sums of the two historians, or rather with wishing to find some confirmation from Herodotus in this particular case, to support the large sum of 1300 in Ctesias, however regardless they have been of unnecessarily setting them at variance in other cases\*. But although it has been one chief part of my object to reconcile antient authors in various cases, where the moderns had without any good reason pretended to find contradiction between them, yet I have shewn above, that the present plan of reconciling Herodotus to Ctesias, would nevertheless render him inconsistent with himself in the rest of his Chronology; so that in fact it would serve only to justify some of the fables of the one historian at the expence of harmony in the other. Yet so

\* Thus Bouhier also observes, " There is one method of reconciling these two authors; for we have only to say, that the kingdom of the Assyrians had subsisted *a long time* without extending itself beyond Niniveh and Babylon; but at length some king of a warlike disposition began to extend the Assyrian empire (as Herodotus says) *over Upper Asia* about 520 years before the revolt of the Medes. I do not oppose this system, which may perhaps remove difficulties at present very embarrassing to Chronologers." *Dissertat. sur l'Histoire d'Herodote* p. 21. *Dijon*, 1746.

fascinating



fascinating has been the plan of reconciling these two authors, and so plausible this method of effecting it, that M. Larcher, in his late French translation has in part adopted the same; and thus by his *augmenting* the sum of Herodotus, and on the contrary *diminishing* that of Ctesias, he has strictly adhered to neither of them, but attempted to make their accounts meet together amicably in the middle between both extremes; and thus form a motley and incoherent mixture out of the two accounts \*. His apology for this is, "that among accounts so very discordant he conceives it impossible to discover the truth, unless some precious work should afterwards come to light, which is more to be wished than expected." *ibid.* But in fact this *precious work* has already been discovered, namely, the work of Syncellus, which has very sufficiently shewn us that the account of Ctesias was indeed faithfully co-

\* "In examining more nearly the text of Herodotus, I think that I perceive this historian to speak only of the time of the *greatest glory* of the Assyrian kingdom: this idea had already occurred to M. Bouhier, p. 16. [This is a mistake, the passage occurs at p. 22] and that he meant only to speak of their most prosperous state, without saying any thing of the sum of years which had *preceded* it." *Chronolog. Herodote. tom. 6. p. 256.*

pied by him from the Asiatics themselves ;  
 but that it contained merely an imaginary  
 astrologic period, like that of the Egyptians,  
 founded on some favorite but inaccurate  
 astronomic principles\* : and that the se-  
 veral reductions and variations made in the  
 original Assyrian sum of 1460 were pro-  
 duced by the Greek Chronologers, in order  
 to reconcile it with their own Greek Chro-  
 nology ; they having been as much puzzled  
 by such a vast period as our own modern  
 Chronologers, and in consequence having  
 contrived as many variations and pretended  
 reconcilements between different accounts  
 as the moderns themselves ; whereby they  
 formed those *discordant sums* of which  
 Larcher complains†. Unfortunately there-  
 fore we find, that the more the moderns  
 attempt to reconcile these discordant ac-  
 counts, the great discordancy they produce,  
 and the *last* writer on the subject, M.  
 Larcher gives up the object as a thing *im-  
 possible* to be obtained : nevertheless the  
*origin* at least of these discordancies may be

\* Appendix, p. 61.

† Viz. 1460, 1400, 1360, 1300, 1280, 1240, 1070,  
 1000, &c. all these have apparently arisen from the  
 original astrologic root 1460, and are all totally in  
 opposition to the other original and historic root  
 of 520.

ascertained,

ascertained, by their being all resolved into these two original sums, the astrologic 1460 of the Asiatic natives, and the historic 520 of Herodotus, as traced out by the Greeks themselves, in computing backward from their own ages by the aid of generations up to the age of Hercules, which was the utmost boundary of Greek computation; and was the same as to say we have gone back now to a dark and unknown age. But as to the present specious method of reconciliation, although favored with the approbation of so many respectable authors, yet it is by no means solid and admissible, for the following reasons. First, there is not the least appearance but that both Diodorus and Appian understood Herodotus to include the *whole period* of Assyrian empire under the 520 years, and not the *last portion* of it only: moreover, the current traditions referred to above, plainly placed Semiramis about the time of the Trojan war, therefore these *could not* have made more than 520 years from the very first *commencement* of the kingdom under Belus or Ninus; why then should not Herodotus also have rather agreed with these two authors and current traditions than have disagreed, unless we knew of some evidence to the contrary? But again,



there is evidence that he did agree with them, whether those accounts and traditions were derived from his computation, or his computation accommodated to those traditions; for he expressly says, that Ninus was the son of Belus, and Belus the grandson of Hercules: consequently, he *must have* placed Ninus about the time of the Trojan war, or a little later only; and therefore *could not* make more than 520 years of the *whole kingdom*, from Ninus down to its dissolution by Arbaces, even although he placed the capture of Troy at full 500 years before the Olympiads, as in fact he did; whereby it appears, that this interval is in perfect harmony with his sum of 520 years for the *whole kingdom*, because it is agreed by all that it was *ended* before the Olympiads. Still farther, this favorite sense is directly *contradicted* by the accounts of Ctesias himself, and all other Greek historians quoted by Diodorus; who all agree, that it was Ninus who had thus extended the Assyrian kingdom *over Upper Asia* at the very *commencement* of that kingdom; so that there can be no pretence for its being effected at a *later* period of only 520 years out of 1300, before its dissolution: they add likewise, that *all* the succeeding kings lived in pleasures and idleness,

ness, so that Diodorus deemed it needless even to mention their names, as *they did no memorable actions* \*. Now can we reasonably in opposition to Ctesias and those others who first adopted that longer period than 520 years, suppose some *warlike king* to conquer *Upper Asia* afterwards, merely in order to extend those 520 years to 1300 or more, and this, although we are informed that it had been already conquered at the very first by Ninus himself? It is plain then, that this present favorite mode of reconciliation is just as unsolid and untenable, as that other less specious one to which it has succeeded; it being neither countenanced by the words themselves in Herodotus of the passage in question, nor by those of Diodorus and Appian, but directly contradicted by another passage in Herodotus, where he makes Belus to be grandson of Hercules; and directly contradicted again by the words of Ctesias also himself, in favor of whose computation this favorite sense above-mentioned has been adopted, in order to reconcile it to that of Herodotus. It may be proper

\* “ Deliciis et ocio ignavo deditus Ninyas Nini filius, ad eum prope modum et cæteri reges, quorum nomina, cum *nil memorabile* gesserint, referre non necesse est.” *Lib. 2.*

how-

however, to confirm still more the *incoherency*, which this mode of reconciliation would produce in the chronology of Herodotus, by examining what he has said concerning Belus being the grandson of Hercules, together with the unsolid objections which have been made to this historic information in that antient author by Gronovius and others.

*That there is no sufficient Reason why Gronovius, and other Authors since, should suppose the Ninus, son of Belus, son of Hercules, mentioned by Herodotus, to have been any other person than the famous Ninus King of Assyria, and Founder of that Kingdom.*

**I**N the very beginning of the 1st Book of Herodotus, he traces back the foundation or acquisition of the kingdom of Lydia to Agron, the son of Ninus, son of Belus, son of Alcæus, son of Hercules\*.

\* “Heraclidarum primus Sardium rex extitit Agron, Nini filius Beli nepos, Alcæi pronepos, novissimus Candaules—Heraclidæ ex Jardani ancilla et Hercule geniti per 305 annos, imperium tenuerunt ab Agrone usque ad Candaulem et per 22 ætates virorum.” *Lib. 1.* It may be here a doubt, whether the



Now on reading this, will not the first *presumption* of every person acquainted with Greek and Roman authors be, that the Ninus in question was the famous king of that name, founder of the Assyrian kingdom? For every reader of antient authors has heard of Ninus son of Belus, king of Assyria, but never heard or read of any other Ninus, son of Belus. If then any critic should dispute the truth of this primary and obvious presumption, he ought certainly to produce some strong *evidence* to the contrary; and yet this presumption has been contradicted, not only without any evidence to the contrary, but even against very strong evidence in favor of it. The first person who laid any foundation for a *doubt* on this subject, was the learned Scaliger, who, though an author of great ability, yet by placing too much confidence in his own conjectural sagacity, has repeatedly led his readers into many errors: in the present case however, he has gone no farther than only just to hint at an apparent, but not real inconsistency, which, as he thought, he discovered in the account

the reign of Candaules be included in the 505 years or not; if it be not, this will raise up the accession of Agron still *earlier*, and shorten the 110 after Belus. Vid. p. 79.

of

of Herodotus concerning Ninus, in the above-quoted passage;\* and it has been Gronovius and other later authors, who were the first that had recourse to a wild conjecture, in order to remove that pretended inconsistency. The inconsistency which Scaliger pretended to discover was this; that if Herodotus placed Ninus *so late* as to be a great grandson of Hercules, this could never *agree* with those other accounts, which fixed Ninus at 1300 years before the *end* of the Assyrian empire. Now what was this more or less than to urge, that the *shorter* duration of the Assyrian kingdom in Herodotus does not *agree* with the *longer* one in Ctesias and Diodorus? This every one easily allows; but both Scaliger and Gronovius were so prepossessed in favor of the *long period* of Ctesias, that rather than allow Ninus to have lived *so late* as after Hercules, and about the time of the Trojan war, they accused Herodotus of having fallen into some great error or inconsistency: an historic error it might

\* “ Quis non miretur Ninum Beli filium unum ex posteris Herculis fuisse [secundum Herodotum], qui annis mille ut minimum Nino Beli filio posterior fuit? Aut est Herodoti aut librariorum error.” Scalig. Canon. Isagog. p. 327. Amsterd. 1658.—Vid. Gronov. Annotat. ad Herodotum.

be, but not an inconsistency; for the consistency of Herodotus, or the contrary, is not to be judged of by the consistency of his account with that of Ctesias, but by his consistency with himself, that is, by comparing the *historic fact* here related of Ninus being great-grandson to Hercules, with the duration of 520 years assigned by him to the Assyrian empire; and with that other *historic fact* preserved by him likewise, of Agron son of Ninus, acceding to the Lydian kingdom 505 years before Gyges. Now all these *three* historic facts I have proved above to be in perfect consistency, both with each other and with the date of the Trojan war in Herodotus, and also with other chronologic computations of that *middle age* in which Herodotus flourished. This ought to have occurred to Scaliger, before he hinted at such a weak objection; but still more to Gronovius, before he started his own unfounded conjecture, by which he attempts to vindicate Herodotus from that objection imputed to him by Scaliger: this conjecture was, that the Ninus son of Belus mentioned here by Herodotus, was not the famous Assyrian king of that name, but rather some later and different person; and the *chief* argument urged by him or others for this was, that



Herodotus does not here call him king of Assyria. This unnecessary conjecture of Gronovius, in order to vindicate Herodotus from an error which he had not really committed, has been greedily seized on and adopted by several later writers: while on the contrary, Freret, Vignoles and others have taken the opposite side, and allowed, that by Ninus son of Belus, Herodotus did actually mean the famous kings of Assyria of those names; which fact therefore it may be next expedient for me to confirm by some evidence, to strengthen the opinion of these two latter authors \*. Now as to the cir-

\* Gronov. Edit. 1615. "Scaliger was scandalized to find a Ninus and Belus among the posterity of Hercules; but why should not these names so celebrated on account of the great actions of those who first really bore them, have spread into other neighbouring states, and have been adopted by other persons afterwards?" *Memoirs de Literat.* tom. 7. *Recherches sur les Rois de Lydie par M. Sevin*, 3 part. 1723.—"Herodotus has not said, that Belus and Ninus, whom he supposes to be descendants of Hercules, were the kings of Assyria of those names; for there were more persons of the name of Ninus than one, and also more of the name of Belus. Vid. Perizon. Origin. Babulon. p. 58 and 93." *Dissert. sur Herodote par Boubier*, p. 17, 1746.—Nay, even the last writer on the subject, Larcher, seems to have adopted the same error; for he thus refers to the accusation by Scaliger, and the refutation of it by Gronovius, "concerning the objection of Scaliger, readers may consult the note on this sub-  
ject

cumstance, that Herodotus does not call those persons kings of Assyria, this might arise, as before observed, merely from his great brevity on the subject of Assyria in this work, because he had reserved all such articles for his other work, expressly containing the history of Assyria: but even allowing some other cause for the omission, yet does not the omission itself rather tend to prove, that he *meant* the kings of Assyria than the contrary? For these were so well known, that it was unnecessary to add any thing to their mere names, to explain whom he meant; whereas, if he had meant any other more obscure persons, he would probably have made some addition, to prevent his readers from falling into an error. Moreover, although Bouhier does indeed rightly say, that there were more of the name of Ninus than one, and also of Belus; yet any other person of the *name* of Ninus, and at the same time *son* of Belus, besides the king of Assyria, is no where to be found mentioned in antient history: therefore they have no right to suppose

ject in the edition of Gronovius." *Tom. 1. Annot.* 24. by *Larcher*, 1786. For authors of the contrary opinion, *vid. Chronol. de Lydie par Freret, tom. 7. ibid.* and the note to my p. 83 above concerning Vignoles.

and purposely to invent such a new person out of their own imagination, merely in order to obtain a proof, that the Ninus thus descended was not the Assyrian king who was so well known. Besides, if there really ever had been any such other person as a Ninus, son of Belus, except the king of Assyria, yet this person must have likewise lived at the *very same time* with that Assyrian king, as he is fixed above by the Chronology of Herodotus: but that *two* such persons of the name of Ninus should have been not only thus *contemporaries*, but both of them likewise *sons* of a Belus; and yet no notice taken of such an extraordinary fact by any antient writer, is so very improbable, as to deserve no farther consideration, than that the conjecture is of such a nature as to refute itself, by its total want of support, both from history and probability. Still farther, it is remarkable, that an account similar to this in Herodotus of an *Agron* being descended from a Ninus son of Belus, is to be found also in Pollux; yet evidently not copied from Herodotus, but from some other historian, since an explication of the name *Agron* is here added, which is not found in Herodotus\*.

\* Ου φαυλον αν ειη προσειπειν, οτι Νινος ο Βηλου του αυτου παιδα εν αγρω τεχθεντα Αγρωνα ωνομασεν. Non ab  
 τε



Now here in Pollux again no mention is made, whether the Ninus son of Belus in question was the well known king of Assyria, or any other person of the same name. But then here again, as before, we may reasonably conclude, that the person in question must have been that king of Assyria, otherwise probably some farther explanation would have been added to distinguish that it was not he who was meant; it being universally customary to speak of eminent persons with brevity, and without particularizing who they were, because they were so eminent, that scarcely any reader could be so ignorant as to want further information: just as when any one says, that some certain event happened in the time of Alexander, all readers immediately conceive, that by Alexander was meant Alexander the Great king of Macedonia; notwithstanding that there were many other Alexanders in antient times, who were kings likewise. But in the present passage of Pollux, it is still more reasonable than in Herodotus to conclude, that the king of Assyria must be meant; because in He-

re verò esset addere, quod Ninus ille filius Beli suum  
 filium in Agro natum *Agrona* nominavit. *Lib. 9.*  
 c. 2.

rodotus

rodotus it was from *necessity* that *son of Belus* was added to Ninus, in order to give his readers the descent of *Agron* from *Hercules*: whereas in Pollux, *son of Belus* was introduced *voluntarily*, and evidently by way of distinction likewise, in order to ascertain what person of the name of Ninus was meant; there having been more than one of the name of Ninus, but no other Ninus recorded, who was *son of Belus*, except the noted king of Assyria, so far as we know. His phrase therefore *Νινος ὁ Βηλου* proves, that in Herodotus likewise as well as in Pollux, it was to Ninus king of Assyria, that Agron was son, and not to any other Ninus; which is farther confirmed by the following circumstance, namely, that since some other historian (whom Pollux copied) had, besides Herodotus, mentioned the descent of a person of the name of Agron from a Ninus son of Belus, this Ninus could not therefore be any obscure person: as indeed appears likewise both by his descent from Hercules, and also by his being the progenitor of the race of Lydian kings; and as moreover he lived at *the very same time*, at which Herodotus fixes Ninus king of Assyria, all these circumstances conspire so uniformly to prove the identity, that it becomes quite extravagant to suppose

pose him to be any different person from that well known king. It seems to me indeed even impossible for any one to read the above passage in Pollux without conceiving, that he is speaking of some persons and facts *well known* in antient history; and they who take upon them to suppose those persons to be different from the well known kings of Assyria, ought first to prove, that any such different persons ever existed. To this may be added, that other evidence has been preserved beside this of Herodotus concerning the current tradition of the kings of Lydia having been descended from Hercules, viz. by Apollodorus;\* so that upon the whole, any objection to my *statement* of these historic facts, arising from the conjecture of Gronovius, that there was some other person named Ninus, rests upon no

\* “ Quem Hercules sustulit filium, ex Omphale Lydorum regina, nomen fuit Agelaus [Alcæus] unde et Cræsi genus.” *Lib. 2.* While this account agrees with that of Herodotus in some things, it differs also enough in others to prove, that it was not taken from Herodotus, but from some other Greek historian, and that current traditions were the foundation for both: consequently there is no more reason to except against that article mentioned by Herodotus concerning the descent from Hercules being through Ninus, than against any other variation between the two different modes of relating the tradition.

good



good foundation: it being nothing more than mere unfounded conjecture in opposition to joint historic evidence; which is at the same time attended with probability and consistency in the accounts of Herodotus, and in some points confirmed by other antient authors.

However, as it is essential to the nature of truth, that it will bear to be surveyed on different sides, without displaying any appearance of weakness, improbability or disproportion between its several parts, it may be expedient to survey the above *statement* of historic facts, dates, and lengths of reigns, after a different manner. We before examined the several *particular* dates, reigns and facts in Herodotus, without discovering any want of harmony among themselves, or with the current accounts of other antient authors; let us now consider the *general average* of *all* the reigns above-mentioned, in order to see whether they agree with that general average, which Herodotus himself allows to a succession of reigns. I have placed the first of the persons in question, Alcæus, at the time of the Argonautic expedition, 80 years before the siege of Troy. This, indeed, is not quite accurate; but what inaccuracy subsists in the date is to the detriment of my  
own

own cause; for Hercules seems to have resided in Lydia after the Argonautic expedition several years before Agron was born; so that by placing his birth full 80 years before Troy, I augmented too much the total sum of years after his birth. I do not mention this in order now to correct it, but shall still consider the *birth* of Alcæus as fixed rightly before. Now we reckoned before 27 years from his birth down to the accession of his son Belus, to the throne of Assyria, (p. 74) according to the sums in Herodotus: but we must deduct about 14 from these 27, in order to begin no earlier than with the *maturity* of Alcæus when he begat Belus, which leaves only about 13 from the *maturity* of Alcæus to the *maturity* and *accession* of Belus to Assyria; and we found that there were 53 more from the *accession* of Belus to the capture of Troy, in all 65. We found also, that there were 56 from the capture of Troy to the *accession* of Agron to the kingdom of Lydia. But at his accession he might have been past his first *maturity*, and even in an advanced age; so that we may at least deduct 20 years on account of this uncertainty from the above 56, and then there will remain only 36 *after* Troy; which when added to the 65 *before* Troy,

amount to 101 years in all from the *maturity* of Alcæus to the *maturity* of Agron, not to his accession to Lydia. Now if the 101 years of these three lives of Alcæus, Belus and Ninus (reckoned only from the *maturity* of Alcæus to the *maturity* of Agron son of Ninus) be divided by three, they give less than 34 years to each life *after maturity*; and *such a life* (viz. from the maturity of a father to the maturity of his son) is nearly equivalent to a *reign*; but if any thing different, it will be rather longer than shorter: therefore if 34 years be on an average too long for a *reign*, yet it will not for a *life* after maturity; especially as some years ought to be deducted from the 65 before Troy, because we placed the birth of Alcæus *too early* by some years; and again, some years possibly ought to be deducted from the 36 after Troy, because it is not certain, whether I have not still placed Agron *too late* at 36 after Troy, as mentioned in note to p. 99. But 34 years even for a *reign* was at least agreeable to the estimation of Herodotus and others in his age, for he allowed only three reigns to every century: hence then we find, that the computations and dates assigned by him to the above personages agree with the *general average* allowed in that age to a *reign*.



*reign.* If it be objected, that such an average is found to be too large for the course of nature in modern times, this is of no moment; because the object in view was to shew, that Herodotus was every where in his computations consistent with himself and with the current mode of estimating the length of reigns in his own age, when the manners of men might be more simple and their lives more active and healthy. But even if 34 years to a reign be too large a sum for any age whatever, whether antient or modern, yet it is hereby shewn to be at least consonant with *reputed* truth, though not with *real* truth; therefore it is an objection only against the current opinions of the age in which Herodotus lived, not against the accuracy of that historian himself: so that nevertheless, harmony is found to subsist between *his own* several dates, sums and computations, notwithstanding that these should be not consistent with the ordinary course of human nature, or with *real* truth. However, it is certain, that in several *particular* examples in even modern times, 34 years, and even 38 years on an average, have been in a few cases found in a succession of *several* reigns; although indeed it is too long for a *general average* of the reigns in

many different nations taken together. What therefore does *sometimes* happen in modern times, might have happened in antient times in the present example. This harmony then, Scaliger, Gronovius and others ought to have noticed, before the former *doubted* concerning the consistency of these relations in Herodotus; and before the latter *invented* his conjecture of the Ninus in question being not the well known king of Assyria, in order to remove that pretended inconsistency, which the *doubt* of Scaliger had suggested, but which does not really subsist in the historic accounts given by Herodotus: and yet no justification of him concerning these articles by any of our Chronologers has hitherto occurred to me in any of their writings; so much more ready are writers to find fault, than to examine whether there be any good foundation for their censures!

Having thus examined that *portion* of my foregoing *statement*, which relates to the sums and historic accounts of Herodotus, and shewn the unreasonable nature of the objections which have been made to them, as well as the no less unreasonable method proposed by Gronovius, and adopted by others down to the last author Larcher, in order to remove those  
 unsolid

unsolid objections to the *short* period of 520 years for the Assyrian kingdom; let us next examine such *portions* of my above *statement*, as relate to the *long* periods of 1300 and 1460 years for the same kingdom, given us by Ctesias and Syncellus: concerning these we shall on the contrary find real and solid objections to the consistency and truth of such accounts, as well as likewise discover a variety of errors made by the moderns concerning them; which have assisted to hide their defects from our observation, and to obstruct our view of the true nature, origin, and antiquity of those long periods; whereby the conclusions of the moderns concerning this subject have been often directly contrary to the conclusions, which would have naturally and necessarily resulted from a *true statement* of facts. But before we quit the former of these two *statements*, it may be proper to subjoin a table of the dates of the several events under consideration.

Before Cap. of Troy.	Before Birth of Herod.	Before 1st Olym.	Before Christ.	
100	900	—	—	Hercules born according to Herodotus.
80	—	—	—	He accompanied the Ar- gonauts, but was left by them



Before Cap. of Troy.	Before Birth of Herod.	Before 1st Olym.	Before Christ.	
80	—	—	—	them in Asia Minor, when about 20 years old.
65	—	—	—	About this time, while he lived in Lydia, Alcæus was born of a servant maid of Iardanus, king of Lydia.
52	—	561	—	Alcæus, now about 14, begat Belus.
42	—	551	—	Belus, now about 13, founds the kingdom of Assyria, in case it ended 70 years before Nabo- nassar, and 40 before O- lympiads: the 27th year from the birth of Al- cæus, 520 bef. end of K.
—	—	—	—	But if the end of the king- dom of Assyria was only 60 years before Nabonassar, the commencement of it by Belus was 37 years after the birth of Alcæus, therefore both Alcæus and Belus were older than 14 and 13, by 10 years be- tween them, which is more probable, although the other is possible.
—	—	—	—	Troy taken, according to Herodotus, <i>a little before</i> the birth of Pan by Mer- cury

After Cap. of Troy.	Before Birth of Herod.	Before 1st Olym.	Before Christ.	
—	800	508	1284	Curry and Penelope: some say, 10 years before.
After Troy. 8	—	500	—	Pan born, according to Herodotus 800 years be- fore his own birth, in 484 before Christ; there- fore, 1284 before Christ.
14	—	494	1270	Ninus begins to reign about this time; 60 years after Belus.
24	—	—	1260	Troy now taken, accord- ing to the life of Homer, erroneously ascribed to Herodotus; which is the date adopted by Larcher.
36	—	—	—	Pan only now born ac- cording to Larcher, 10 years after his own date of the capture of Troy, and 800 years before He- rodotus was 24 years old, during his travels into Egypt in 460 bef. Chr.
38	—	470	1246	Agron, about this date, might be at maturity of age.
				Troy only now taken ac- cording to Thucydides, if it was taken only 130 years before Nileus led the colony of Ionians in- to Asia, at which time Melos

After Cap. of Troy.	Before Birth of Herod.	Before 1st Olym.	Before Christ.
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Melos was founded; from which foundation Thucydides reckons 700 down to the 16th year of the Peloponnesian war, which was in 416 before Christ: to this date add 130 and 700, they place the capture of Troy no earlier than the 1246th before Christ. But if with Larcher we make 154 years from the Ionian colony of Nileus and foundation of Melos, back to the capture of Troy, they fix it in 1270 before Christ, the 494th before 1st Olympiad, as in the above life of Homer. But the antients generally made less than 130, and none of them more, between those events; therefore there is no good foundation for Larcher's augmenting the interval to 154, nor consequently any foundation for his supposing Thucydides to place the capture of Troy in



After Cap. of Troy.	Before Birth of Herod.	Before 1st Olym.	Before Christ.	
				in 1270 before Christ, which is 24 years at least <i>sooner</i> than the dates of Thucydides really point out.
56	—	—	1227	Agron, son of Ninus, now acquires the kingdom of Lydia, but at what age is not known; 505 years before Gyges reigned in Lydia according to Hero- dotus: but if Herodotus did not include Can- daules in the 505, Agron acceded much <i>sooner</i> .
80	—	—	—	The Heraclidæ conquer Peloponnesus, according to Herodotus's date of the capture of Troy.
130	—	—	1154	Nileus now led a colony of Ionians into Asia, ac- cording to the date of the capture of Troy by Herodotus.
168	—	—	1116	Nileus only now led the Ionians into Asia accord- ing to Thucydides; and now Melos was founded according to him, 700 years before the 16th year of the Peloponnesian war. For the extract from Co- non

After Cap. of Troy.	Before Birth of Herod.	Before 1st Olym.	Before Christ.
—	—	41 end of Assy- King.	—
508	—	1 Oly.	776
—	—	14 Oly.	722

non in Photius (sect. 46) says, that Melos was founded at the *same time* with the colony of Ionians by Nileus; which if at only 130 years after Troy, these 130 added to 1116, carry back the capture of Troy no higher than to 1246 before Christ, not to 1270. But Larcher places the colony under Nileus, and the foundation of Melos 154 years after Troy, which added to 1116, would carry back the date of the capture of Troy to 1270: 154, however, is more than any author allows, whether antient or modern. Therefore Thucydides must have placed the capture of Troy 14 years at least later than the life of Homer, not at the same date of 1270 before Christ; and 38 years later than the real date of Herodotus, or more.

Gyges reigns in Lydia according

After Cap. of Troy.	Before Birth of Herod.	After 1st Olym.	Before Christ.	
—	—	—	714	According to my table.
—	—	—	484	Vol. II. p. 217.
—	—	—	460	Gyges begins in Petavius.
—	—	—	416	Herodotus born 4 years before Xerxes crossed the Hellespont.
—	—	—		Herodotus travels into E- gypt at 24 years of age, as Larcher supposes.
—	—	—		The 16th year of the Pe- loponnesian war, the 700th after the founda- tion of Melos, by emi- grants from Greece, who left it <i>at the same time</i> that Nileus led a colony of Ionians into Asia, as Co- non says in Photius, cod. 186, sect. 36 and 47.

☞ Observe, that at p. 74 above, near the bottom, there is an error at these words, "that the accounts of Eusebius and Syncellus, &c. *as far as to* would extend." Instead of this sentence correct it thus: [that the accounts of Jerom and others place the *end* of the Assyrian kingdom only 60 years, and even less before Nabonassar, instead of 70; which variation, by fixing the *commencement* of the Assyrian kingdom 10 years or more *later* than before might extend, &c.] Now in proof of this, Jerom says, "In diebus Oziæ regis Judæ, quando apud Assyrios regnabat Sardanapalus, et apud Latinos Procas Sylvius." *Not. Ap. Amos, c. 1.* Thus he makes Ozias, Sardanapalus and



Procas to be cotemporaries : but this he could not copy from Eusebius, but from some other Chronologer; for Eusebius fixes the *end* of Sardanapalus in the preceding year before Procas *begins*: therefore Jerom and those others must have brought down the *end* of the Assyrian kingdom about 10 years later than Eusebius, i. e. to 60 instead of 70 before Nabonassar; for there Petavius fixes the *beginning* of Ozias, though the *end* of Sardanapalus *sooner*. To this correction is accommodated the paragraph above, at 42d year before Troy. According to my table, v. ii. 216, Ozias began four years still later.

*That various Errors, into which modern Chronologers have fallen, with respect to the less Sum of 1300 as well as the longer one of 1460, for the duration of the Assyrian Kingdom, have been the chief Causes of the different Opinions, false Conclusions, Uncertainty and Confusion which have hitherto obscured this Subject—but that when all these Errors are corrected, such Light and Harmony will be diffused over it, as will sufficiently ascertain, that 1460 was the antient, original, and genuine Period, at first communicated to the Greeks by the Asiatics themselves; which, however, was afterwards reduced to 1300 by the later Greek Chronologers, in order to correspond with a similar Reduction of 100, or nearly 160 Years, which they had made in their own*

*own Greek Antiquities, particularly in regard to the Date of the Trojan War, reduced by the same Chronologers from above 508 to 408 years only before the Olympiads.*

**A**S we have found the short period of 520 in Herodotus to be both consistent with probability, with his own computations elsewhere, and also with such other historic accounts of those early times, as have been preserved; so on the contrary we shall find the longer periods of 1400, 1360, and 1300, to be improbable in themselves, and also to have no apparent foundation whatever in history and Chronology, but to be rather mere abridgements by the Greeks, of the original Asiatic period of 1460; which last itself seems formed solely by the astrologic superstition of the natives of Asia, in order to include the duration of the Assyrian kingdom within an astronomic period of 1460 solar years. This would long ago have been perceived to be the true state of things, relative to these several vast periods, if the moderns had not fallen into a variety of errors, either in facts, criticism or reasoning; which have consequently both hid the right view of the subject from themselves, and involved their readers likewise in more confusion than

than at first. By pointing out therefore these *errors*, I shall at the same time confirm such *portions* of my foregoing *statement* of facts, as might otherwise be subject to doubt, and therefore may require illustration.

Now their first *error* was the result chiefly of accident; for it unfortunately happened, that the principal information, which the moderns had or could obtain concerning the duration of the Assyrian period, was from several of the *later* Greek and Roman authors, such as Justin, Eusebius, Augustin, Agathias, &c. who all agreed in about 1300 years; this established an union of *authority* in favor of that sum.\* There was one antient author indeed, Ctesias, who had assigned the longer sums of 1400, or 1360 years, according to some

\* "The most agreed continuance of the Assyrian kingdom I find to be 1300 years—I prefer this number far before 1460." *Cumberl. p. 192, 198.* "Those who assign but about 1300 years, are Eusebius and Augustin. Justin, from Trogus, expresses it by 1300 precisely. Diodorus, I confess, hath 1360; but I have great reason to believe it was otherwise in the antient MSS. for this place of Diodorus is cited twice by Syncellus, and both times as being 1300. The same place is also quoted by Agathias as being 1306, whence it is probable, it was changed by copiests into 1360." *P. 192 and 3.*

extracts



extracts from him preserved by Diodorus; but although he lived much *earlier* than those others, (he being near a century before Alexander) yet little credit was thought due to his accounts, both because he seemed to contradict himself (although in fact he did not) by assigning 1360 in one passage and 1400 in another; and also because he had the reputation of a fabulous writer, who was too desirous of magnifying Asiatic antiquity, and also of contradicting Herodotus: his accounts were, moreover, not confirmed by any other antient now extant; whence it was concluded, that the Greeks and Romans thought as little credit due to him as the moderns. Hence the sum of 1300 gained possession in public favor, though taken from *later* authors; while that of 1400 from Ctesias was exploded, though communicated by a *more antient* author, as well as one apparently more able to come at the truth, through his long residence in Asia; and an author likewise, who, as we shall find, did really communicate the true Asiatic account, but though the above-mentioned accidental circumstances, was disbelieved: thus the joint authority of *several* much later retailers of antient facts, got the better of a *single* more antient and original historian, whose

whose defect of dealing in the marvellous, was not in this case at least his own; for he only faithfully related the marvellous fables which the Asiatic nations had themselves invented.\*

There was a farther accidental circumstance which strengthened the *authority* in favor of 1300, and which arose from Christian Chronology; for 1300 seemed to agree best with the date of Noah's flood in the Jewish scriptures, according to the shorter computation of it in the Hebrew text; hereby the foundation of Babylon by Nim-

\* Nay, so prejudiced have the moderns been against the fums of the Assyrian period by Ctesias, that rather than adopt them, they have questioned the accuracy of the MSS. of Diodorus, in which those extracts from him were preserved; sometimes pretending, that 1360 was an error in the MSS. for 1306, the sum in Agathias; and at other times, that Syncellus quotes Diodorus as assigning only 1300, not 1400, which proves, they say, the present MSS. to be corrupted. Vid. Petavius, lib. 9, 13, and Cumberland, p. 193, Marsham, sect. 17, 521. But in fact there are so many manuscript errors in the dates of Syncellus, that to correct those of Diodorus by his, is to correct a better evidence by a worse; besides, that there are not above two MSS. of Syncellus extant to be collated together. Still farther, the same Syncellus has preserved an account of the Assyrian period having been still longer, viz. 1460 from Belus, which confirms the testimony of Ctesias in assigning 1400 from Ninus only.

rod (who was supposed to be either Belus or Ninus) would be fixed about 150 or 200 years later than the flood, which was considered as not inconsistent with probability: \* whereas 1400 would have made the commencement of the Assyrian kingdom by Belus to have been coeval with, or within 50 years after the flood; which was rightly considered as absurd, and 1460 would have made the case still worse.† The same motive also gave 1300 a preference to the 520 in Herodotus, which would have been much more too short for the identity of Belus with Nimrod, than 1400 was too long.

\* “Justini numerus 1300 cum scriptura et cum æra Babylonis apprimè *convenit*—hoc modo Belus, qui Babylonem condidit, fuerit Nimrod, quem scriptura primum regnasse apud Babylonem refert.” *Helvicus*, p. 5. “By rejecting a longer sum (than 1300) we take away a great objection against the flood, according to the Hebrew account.” *Cumb.* p. 203.

† “A longer period than 1300, would place the beginning of Belus before the flood, stated by the Hebrew numbers; wherefore it was necessary, that I, who defend the Hebrew numbers, should give such reasons, as I have done, against any such longer series of kings at Babylon.” *Cumb.* p. 203. “Qui numeris 70 interpretum adhæferit, *absurdum hoc*, quasi Assyrii reges in Aquis diluvii vixerint, evitat, etiam si illis 1460 vel 1400 annos adjudicat; miror ergo Scaligerum qui Hebræorum numeros retinet, summam 1460 adeo laudasse.” *Strauchius*, lib. 4.

c. 4. qu. 5.

VOL. III.

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Thus



Thus an unfounded presumption, that the profane Belus was the same person as Nimrod in scripture, after the deluge, confirmed the predilection for the *medium* sum of 1300, which other motives had conspired to excite; and thus a succession of able men, Calvisius, Petavius, Helvicus, Capellus, Sträuchius, Cumberland, Vignoles, Lloyd, Prideaux, &c. were misled by precarious reasonings, founded upon more precarious facts, to hold fast that established error of 1300 being the true sum of the Assyrian kingdom: this too, even *after* as well as before that new and important fact, which the discovery of the MS. of Syncellus first communicated to us, namely, that the most antient and original sum of that kingdom, as given by Berosus and the Asiatics themselves, was still longer than either of the longest sums in Ctesias, i. e. 1460 years. Neither did the circumstances, which naturally accompanied our knowledge of this new and still longer period of 1460, abate the religious ardor in favor of 1300, but on the contrary, the moderns moved heaven and earth to get rid of this longer period of 1460, just as they had done before by those of 1360 and 1400 in Ctesias; of which examples may be seen in Petavius and Cumberland:

berland:\* and yet it was now become still more evident than before, that there were not the least traces of consistent and real history and chronology, either sacred or prophane, to be found in *any one* of these Assyrian periods, but that they were solely founded upon a fanciful astrologic computation, of the Assyrian kingdom being contained in the well known and favorite astronomic period of 1460 years, to which the Asiatic nations had been accustomed as a mode of computing past times. Nay, even those few authors who did see the

\* "I shall offer my reasons against this series of 41 kings [reigning 1460 years.] 1. Because I can find no authority extant to justify it." *Cumberl.* 198. Yet it is very evident from the words of Syncellus, that he copied both the royal names and sum itself from Berofus and Polyhiflor, who were the next authors in point of antiquity to Ctesias, whose sums they have thereby confirmed; whereas the other authors, who assign only 1300, lived much later than the above three, viz. Justin, Eusebius, Augustine, Agathias. The fact then is not true, that there is no authority for it, there being the better authority of the more antient authors, Ctesias, Berofus and Polyhiflor against three or four *later* authors, who did not flourish until 500 years after the former; and who had, moreover, then found out reasons for adopting the mutilated sum of 1300 in preference to the genuine one 1460, viz. because it agreed best with the *reduced* Greek antiquities, as well as Jewish ones after the deluge.

truth, after the MSS. of Syncellus came to light, that 1460 was the real and original Assyrian period, before it was reduced by Greek Chronologers; yet by still giving *historic* credit to it, and attempting to reconcile it to Jewish history, they have either fallen into the absurdity of Scaliger, in placing the foundation of that kingdom by Belus as early as the flood, according to the Hebrew computation; or else like Pezron and Riccioli (the only other two authors who have adopted 1460 years) have still attempted to ascertain and support the veracity of the accounts in the Jewish Bible, by the accidental and fanciful duration of a mere astrologic period; at the same time that they rightly got quit of the absurdity of Scaliger, by adopting the more early computation of the flood according to the Greek Septuagint. But Jewish history had better be left to its own internal evidence in the Bible, than to be propped up by such dangerous external supports, as prophane fables and astrologic cycles. An indiscreet zeal of forcing every thing into the service of one's cause, has done injury to many a good one, which would have been more convincing without such foreign and precarious assistance. Accordingly, Archbishop Usher has rejected  
all



*all* the above periods of the Assyrian kingdom, and he alone, I believe, of all modern chronologers; he rightly judged, that it could prove of no detriment but rather advantage to the veracity of the Jewish antiquities, to be deprived of all such weak and fabulous supports from prophane computations. He therefore reduced the duration of the Assyrian kingdom to the 520 years of Herodotus; in doing which, however, he likewise fell into two *errors*, one of which I have pointed out at p. 83: and another was in making the Assyrian kingdom end about 70 years too late, namely, not before the rise of the æra of Nabonassar, 29 years *after* the 1st Olympiad, for which he had no authority except a very doubtful one from Paterculus; whereas many antient testimonies, both profane and Christian, concur in fixing its end at about 40 or 50 years *before* the Olympiads, and some much sooner.\*

The event, however, which first occurred to shake the credit of this sum of 1300

\* Vid. p. 45 above. Those 520 Assyrian years ought however to have been reduced to 420, in case the 508 from Troy to the 1st Olympiad be reduced, as they are by Usher to 408, agreeably to what is shewn at the above p. 83. If the reasons for one of them are good, they are equally applicable to the other.

years, to correct the *errors* into which Chronologers had fallen concerning it, and to pave the way for the total rejection of *all* the long Assyrian periods by Usher in 1650, was this discovery of the MS. Chronology by Syncellus, of which Scaliger first made public some extracts in his edition of Eusebius, in 1606. For in this MS. the public obtained the first information, that even a still longer sum than the 1400 of Ctesias, had been attributed by some of the antients to the Assyrian kingdom, viz. 1460 : Scaliger, at that time the king in literature, immediately and rashly adopted it ; for he adopted it without any other reason, than because he supposed, and only supposed, that it had been the sum adopted by Africanus, from which Christian chronologer he again supposed, that Syncellus had copied it ;\* for which two suppositions however, he had not the least good foundation, and neither of them are apparently true. These hasty suppositions led him into another *error*, and indeed a strange absurdity ; for he at the same time still adhered to the current preference

\* " Hos reges 41 Assyriorum [et annos 1460] Africano luculento scriptori debemus." *Canon. Isagog. lib. 3. p. 321.*

of that age for the shorter computation of the deluge, in opposition to the longer one of the Greek septuagint; whereby on thus carrying back the commencement of the Assyrian kingdom so far as 1460 years, he made it to begin *during* or *before* the deluge, as above-mentioned. It was this absurdity of Scaliger and the necessary incoherency of the new Assyrian period of 1460 from Syncellus, with the computation of the deluge according to the Hebrew text, which cast a damp at first upon its reception as the real sum of the Assyrian kingdom; because all men in that age were infatuated with the shorter computation of the deluge in the Hebrew text, as before mentioned, with which 1460 would not agree. In this therefore, even Calvisius, otherwise a close follower of Scaliger, deserted him: nevertheless, Riccioli soon afterwards revived and adopted 1460, but then he avoided the absurdity of Scaliger, by adhering to the more early computation of the deluge, according to the septuagint.\* He was followed by Pezron in 1687,† who, in course, strenuously joined with

\* Chronologia reformata, Bononiæ, 1669. Tom. II. p. 3.

† L'Antiquité des temps rétablie. 1687.

Riccioli



Riccioli in supporting the *new* opinion, then almost heterodox, which Vossius had started in 1661, concerning the preference of the septuagint computations to those of the Hebrew text.\* Since these Chronologers, all others have received again the more *antient* opinion in favor of the shorter computation of the Hebrew text, and consequently the shorter Assyrian period of 1300; until Jackson, in 1752, revived once more the question concerning the preference of the septuagint computations, which has very much since gained ground, and received the approbation of Kennicot. Jackson in consequence revived likewise the longer Assyrian period of 1460, yet with some difference however from those former authors; for he did not reckon that sum down from Belus, whom many persons suppose to be Nimrod, but from a more antient king at Babylon, called Evechous, long before Belus; of whom Syncellus had given an account from Polyhistor, and also had supposed to be Nimrod, in which opinion Jackson followed him: so that he reckoned only 1305 after Belus; however, his 1460 after Nimrod or Evechous, is

\* De septuaginta interpretibus chronologia. Hagæ-com. 1661.

still equally inconsistent with the shorter computation of the deluge in the Hebrew text, though not with that of the septuagint. Thus we see that opinions have changed with the winds, and the new period of 1460, first communicated by Syncellus, but from very antient *profane* authors, has depended for its reception upon *scriptural* computations, that is, upon the preference or the contrary of the septuagint to the Hebrew text. Yet, in reality, the Assyrian period is totally independent of this scriptural question, except with those who attach *historic* credit to it; which I myself do not, being convinced, that it is merely an astrologic period of the Asiatic natives, yet nevertheless the original and genuine sum of the Assyrian kingdom, which they had at first communicated by Ctesias and Berosus to the Greeks: but which the Greeks afterwards reduced to 1300, in order to make it agree the better with their reduced date of the capture of Troy, from more than 508 to 408 years; whereby 100 or near 160 years were cut off from both computations *after* the siege of Troy. But notwithstanding that these moderns have thus reprobated and removed the *error* and absurdity of Scaliger in one point, by their adoption of the *more early*

VOL. III. T date

date of the deluge from the septuagint, along with the *longer* Assyrian period of 1460; yet they have *all*, nevertheless, down to this very day, still blindly followed Scaliger in another of the above *errors*, and apparently without even examining the question, that is, in supposing that Africanus had adopted this period of 1460, and that Syncellus had copied it from him: nay, so implicitly have all modern Chronologers received this error of Scaliger, that I do not know of any one, who has so much as expressed the least doubt about it, except that Cumberland does indeed take notice, that the period of 1460 ascribed to Africanus makes him inconsistent with *himself*.†

\* “The times affixed to the Assyrian kings in Syncellus [1460] which *he reports to be by Africanus's authority*, have thereby gained reputation with many.” *Cumb. p. 202.*—“The catalogue of Africanus [1460] exceeds that of Eusebius by 220 years.” *Vignoles, Vol. II. p. 166.*—“The whole duration of the Assyrian empire was something more than 1300 but Africanus made it 1460.” *Jackson, Vol. I. 266.*—In the same manner Strauchius, Riccioli, and all others follow Scaliger, in ascribing the 1460 in Syncellus to the authority of Africanus, and even affirm like Cumberland, that Syncellus says so, although in reality it is only an erroneous assertion of Scaliger.

† “I think we may conclude, that we must not state the duration of the Assyrian monarchy by the series of reigns, which is given us as Africanus's

[1460]



*That Scaliger, and all later Authors are in an Error, in supposing the period of 1460 in Syncellus to have been copied by him from Africanus, whose real Sum of the Assyrian kingdom did not in fact exceed 1300 at most, as may be shewn from comparing several other Dates of Africanus, quoted in Syncellus; neither is any thing to be found in Syncellus, which can give countenance to this universal Error, but quite the Contrary: for both the Arrangement and Words of that Author testify, that he copied the Period of 1460, and all his other Assyrian Accounts, either directly from Berossus, or else from the Extracts, which*

[1460] because it makes him inconsistent with himself." P. 202.—"The latter accounts attributed to Africanus, make him inconsistent with himself." P. 204.—This is indeed true, and Cumberland rightly perceived, that if 1460 was the sum of the Assyrian kingdom in Africanus, it was not consistent with some other computations, which Syncellus expressly quotes as being copied from him, and which I shall accordingly point out. I may here observe likewise, that the Assyrian period in Syncellus, is by Scaliger, and sometimes by later authors, said to be 1484 instead of 1460. But this is another error of Scaliger, who assigns to the 25th Assyrian king 24 years more than is found in the MS. of Syncellus himself, whereby the total is increased to 1484. *Vid. Scaliger Animadv. in Eusebium, Ed. 2d. Ann. 1658. p. 47.*

*Polyhistor had made from Berosus in his Historic Collections, about 250 Years after the Age of that Historian and Alexander. So that the Period of 1460 from Berosus is the most antient Account extant after that of Ctesias, with which it chiefly agrees; for all these Three Authors lived 4 or 500 Years before any of those, who make mention of 1300 only, such as Justin, Eusebius, &c.; consequently, it must be the original and genuine Sum, communicated at first to the Greeks by the Asiatics themselves.*

**I**T is the more necessary to prove, that the sum of 1460 was not copied by Syncellus from the Christian Chronologer Africanus, and also that it was copied by him from the profane historians Berosus and Polyhistor, because some moderns, through their unreasonable predilection for 1300, have been idle enough to suppose, that Africanus, about the year 220, or Syncellus about 800, might possibly have themselves then first invented this longer period of 1460, which has been thus ascribed to the Assyrian kingdom. This is, indeed, a very preposterous supposition of itself; for as it was the object of all the first Christian Chronologers, Tatian, Clemens, Eusebius, and even of Africanus himself,

himself, to prove, that the Jewish antiquities were as early, or more so, than the antiquities of the Greeks and all other nations, what motive could those later Christians, Africanus and Syncellus have for the directly contrary conduct in this single case, of augmenting by a forgery of their own the antiquity of the Assyrians; which was already by the period of 1300 carried back almost to the deluge, and by this farther addition, was rendered even prior to it? Such a suspicion, however, although indeed totally improbable of itself, will be effectually removed with respect to Africanus, if I shew, that 1460 was not the sum in that author, nor more than about 1300 at most; and with respect to Syncellus, it will be equally removed, if it shall appear, that this sum was derived by him from the Asiatic historian Berosus, who received his accounts from the archives and traditions at Babylon itself.\*

\* Berosus se tempore Alexandri vixisse refert in primo libro de Babyloniis; plurimorum vero autorum commentaria spatium circiter myriadum annorum quindecim in Babylone summâ curâ asservari; illa vero historias de Cælo, Mari, primo rerum ortu, *regibus* et eorum gestis continere, &c." *P.* 28.  
 " Hæc Polyhistor, Berosum sequutus primo libro refert: secundo vero decem Chaldæorum reges 120 saeos, i. e. annos 432,000 regnasse, usque ad diluvium.



Hence it will in consequence appear likewise, that 1460 was the original, genuine, and most antient sum of the Assyrian kingdom, as communicated by Ctesias and Berosus to the Greeks; the knowledge of which was obtained by them from the information of the Babylonians themselves and other Asiatics: now for what purpose, and by what persons it was afterwards shortened, we shall likewise with great probability

vium. Idem Polyhistor Chaldæorum relationibus eruditus narrat, &c." P. 30. Here we find Syncellus expressly informing us, that he received his most *early* accounts concerning the Assyrian kings, either directly from Berosus, or from his copiest Polyhistor, or both: this forms a *presumption*, that he copied his *later* Assyrian accounts from the same authors; and accordingly to those authors, he often refers afterwards, but never quotes Africanus for Assyrian history, and only for Egyptian and Jewish, which had some connection together; but the Egyptian accounts of Africanus are expressly said to be taken by him from Manetho; neither does it any where appear, that Africanus had ever inserted any regular catalogue of the Assyrian kings, it is only probable; because he begins his catalogue of Egyptian kings much earlier than Belus reigned in Assyria. Now, because this is probable, had Scaliger, Jackson, and the rest any sufficient ground for ascribing the Assyrian accounts in Syncellus to Africanus, while Syncellus himself repeatedly thus quotes Berosus, Polyhistor, Abydenus, and other prophane historians for his authority, but never quotes Africanus for any of them?

be able to conclude, if we consider, that four contiguous reigns, amounting to 162 years, are found omitted in those catalogues of Assyrian kings, which make only 1300 years in all; and that this omission is made immediately after that Assyrian king *Teutamus*, who, as *Asiatic tradition* says, was contemporary with the Trojan war.\* Hereby the accession of that king and the Trojan war are placed at about 408 years before the first Olympiad, agreeably to the computation of such *later* Greek Chronologers, as reduced the interval between those two events to the above sum of 408 years. These circumstances and coincidencies speak for themselves; and shew clearly enough, that it must have been the original period of 1460, which has been *reduced* to 1300, and not contrarywise, the latter sum *augmented* in later times to the former; because it was only the *later* Greeks, who reckoned so little as 408 years from Troy to the Olympiads, while Herodotus, Thucydides, and other still earlier ones reckoned 508 or near 550: so that no author of those *later* times could have any motive to make

\* “ Teutamo regnante Græcos Trojam cepisse fertur—multos Græcorum Memnonem trucidasse—hæc de Memnone in regiis commentariis contineri Barbari asserunt.” Diod. lib. 2.

an interpolated *addition* of four reigns of 162 years *after* the Trojan war, in order to make the reign of Teutamius agree with the longer computation of 508 years by the *earlier* Greeks, which was then exploded; but on the contrary, such a reduction of 162 years, and in that very place *after* the Trojan war, was *necessary* to those who reduced 508 to 408. Moreover, while these circumstances excite a strong *presumption* for what purpose, and by whom the Assyrian period was reduced to 1300, the words and arrangement of Syncellus exhibit likewise *sufficient proof*, that it was from the more antient Berosus, that he obtained the knowledge of the original and longer genuine sum of 1460. Thus the *authority* for 1460 will stand upon better foundation than before, which was only upon the *supposed* testimony of the Christian Africanus, but now upon that of the three more antient profane authors, Ctesias, Berosus, and Polyhistor his abridger: while the shortened sum of 1300 is testified to only by the Roman authors, Justin, Eusebius, Africanus, Augustin and Agathias, who lived 5 or 600 years later than Berosus, and also than those Greek Chronologers, Ephorus, Timæus, and Eratosthenes, who were the authors of the reduction



duction of 508 after Troy to 407 only. It was this reduced system of Greek antiquities, which was most in vogue, when that *later* class of Roman authors flourished; whereas Ctesias flourished long before it was made; Berosus, indeed, not until after Ephorus, yet still before his reduction of 508 to 407 was currently received by the Greeks, or confirmed by the approbation of Timæus and Eratosthenes. It must then be thought a very fortunate circumstance, that Syncellus, although he lived still later than all the above antients, namely, not until about 800 years after Christ; and although the MS. of his work was not known of until Scaliger made it public so late as 1606, should have nevertheless preserved the memory, and he alone, of the most antient and original period attributed to the Assyrian kingdom; by his having fortunately taken his account of it from the fountain head, that is, from such very *antient* historians as the Asiatic Berosus co-temporary with Alexander, or his copiest Polyhistor: while on the contrary, all such *later* authors as Justin, Eusebius, &c. above 500 years after Berosus, by copying their Assyrian accounts from such *later* Greek writers as flourished after the *reduced* Greek antiquities came into fashion, under the

*successors* of Alexander, have in consequence made known to us only that reduced and mutilated Assyrian period, which those *later* reformers of Greek Chronology had then first introduced. One cannot therefore but wonder, that Cumberland should fall into such an *error*, as to represent this later class of authors in Roman times, as the *most antient* writers, on the subject; this error produces a confusion, which runs throughout all his tract.\* If then this period of 1460, although the *latest*, which came to our knowledge on account of the late discovery of the MS. of Syncellus, was however copied by him from *more antient* historians, than those who testify to the shorter sum of 1300; there cannot be now the least room for suspecting, that it has been augmented in later times by any forged insertion of four additional Assyrian kings: but the directly contrary must be the real fact, for as the longer one 1460 had existed before the shorter sum of 1300, it must have been *reduced* to this shorter

\* "It is more reasonable to stick to those *elder* accounts, which do not contradict the Hebrew numbers [1300] than to take the *latter* account attributed to Africanus, [1460] which contradicts the *elder* historians, and are supported by no testimony of authors who lived *before* Africanus." P. 204.

sum in times later than the more ancient Ctesias and Berosus flourished; by a retrenchment of four reigns, amounting to 162 years, which would in course shorten 1460 to about 1300. Let us then enquire which of these two was the *real* sum in Africanus, that we may get free still farther of all suspicion of his being concerned in any such pretended *extension* of this Assyrian period to 1460, by proving, that 1460 was actually not the sum assigned by him, but 1300 only, or less.

At p. 63, Syncellus gives an account of the time when Moses lived *according to Africanus*, viz. “When the Ogygian deluge happened in Greece, in the reign of Phoroneus, the 2d king of the Argives, Inachus being the first, which deluge Africanus fixed precisely in the 55th year of Phoroneus, when Moses was 80 years old.”\*

\* “Inachi Phoroneique priorum regum Argivorum ætate Mosem vixisse constat, quum primum illud Ogygi tempore diluvium, anno Mosi 80, et Phoronei regni 55 contigit, *ex mente Africani hæc verbo tenus scribentis.*” Syncell. p. 63.—“Ab Ogygo ad Olympiadem im. 1020, hinc ad Cyrum Olympiade 55 sunt 215; ab Ogygo itaque ad Cyrum 1235.”—“Annis 300 et plus ante Ogygum Ninus regnavit primus.” [*πρωτος ηρξεν Νινος ετις τ πολυ προτερον Ωγυγου.*] Here the editor for τ [300] would read υβ [52], and so he erroneously translates the  
U 2 sentence;



Syncellus then goes on to reckon " 1020 years from the Ogygian deluge to the 1st Olympiad, and 215 more thence to the 55th Olympiad, when Cyrus began his reign; in all, 1235 from Ogyges to Cyrus:" (and he adds) " that Ninus began to reign 300 years and more before Ogyges." Now that the whole of this computation belongs to Africanus as well as the first sentence is confirmed by the very next page, but more clearly still by the following words at p. 148; where Syncellus says again, " Africanus writes thus in his 3d book, that Ogyges lived at the time, when Moses led the Israelites out of Egypt, and after this he adds [*καὶ μὲν ἔτεσσι*] from Ogyges and Moses to Cyrus were 1235 years." Eusebius agrees, that this was the real computation of Africanus, and says moreover, that he copied it from Acusilaus.\* If therefore Africanus placed Ninus at no more than 300 years before Ogyges, these added to 1020, would make no more than 1320 years from the first of Ninus to

sentence; but it will appear afterwards by another computation of Africanus, that 300 in the Greek text is right. Thus editors, by their hasty conjectures, corrupt the text of antient authors!

\* " Africanus Acusilaum ab Ogyge ad 1 Olymp. annos 1020 collegisse refert." *Præp. Evang.* x. 10.

the first Olympiad, consequently less than 1300 to the end of the Assyrian kingdom, 40 or 50 years before the first Olympiad. Now that 300 in the Greek text is right, may be proved by the following computation at p. 125, where Syncellus says, "that he disapproves of the reckoning of Africanus, who in his 3d book writes, *that the kingdom of Argives under Inachus began in the 200th year of the Assyrian kingdom, and in the reign of the 5th king Arius.*"\* To these 200 add the 56 of Inachus and 55 of Phoroneus, they amount to 311 down to Ogyges; and added to the 1020 after Ogyges, the total will be 1331 only from Belus to the first Olympiad, therefore again less than 1300 to the end of the Assyrian kingdom, and also *more* than 300 down to the Ogygian deluge, as he said before: and that there is no error in the numeral 200 is proved by this addition, *of Arius the 5th king.*† On this computa-

\* "Non placet Africanus 3tio historiarum libro scribens, Argivorum regnum cepisse ab Inacho 200 anno Assyriorum, tempore quinti regis Arii." P. 125.

† For Belus reigned 55, Ninus 52, Semiramis 42, Ninyas 38, Arius 30, in all 217 only, to the end of Arius, the 5th king reckoned from Belus, and only 187 to the beginning of Arius; so that Africanus must in this case have reckoned Belus the first, although

tion of Africanus, Cumberland rightly observes, " That as Syncellus blames him for it, this proves the fact, that Africanus did thus compute and affirm; Syncellus indeed thought him mistaken in it, yet he hath not proved the contrary." P. 201.\*

though in his former sum he reckoned from Ninus: there is not indeed quite difference enough between the two sums, 1320 and 1331; however they correspond near enough for our purpose, that is, to prove, that Africanus did not adopt the sum of 1460, but only about 1300 more or less. And still farther, Syncellus may have writ 200 *anno Assy. sub Ario* instead of *sub Ario 200 ann. Assy.* by which Africanus might mean, that Inachus began under Arius, who reigned in 200th year of Assyrians, so that Inachus might not begin until near his end. Now if Africanus, like Eusebius, (*præf. fin.*) gave 65 to Belus, there would be 197 (nearly 200) to Arius, and 30 more to his end, would with 56 and 55 be 341, which added to 1020, make 1361, much longer than 1331.

\* But Cumberland ought to have added, that Syncellus attempts to prove the contrary; it is, however, by a false reasoning, from which no other conclusion follows, than that the above sum of the Assyrian empire would be very inconsistent with some computations of Syncellus himself; yet even this confirms still more, that there is no error in the numeral 200. The false reasoning is of the same kind as that of Vignoles, at p. 83 above, i. e. he shews an inconsistency, which will follow from mixing together the dates of two different systems. This inconsistency is, that hereby the exit of Moses would be only 100 years later than the birth of Abraham was computed by Syncellus himself. But  
though



Upon the whole then, the dates in all these *three* passages of Syncellus correspond with each other in proving, that Africanus allowed only about 1300 years to the Assyrian empire, not 1460.

It remains still to enquire, from what authors Syncellus did copy this period of 1460, since it was evidently not taken from Africanus. Now in truth it is wonderful, that the moderns should have gone on so long and so blindly in ascribing this period to Africanus, although Syncellus gives such sufficient and repeated information of its being copied either directly from Berosus, or else through the medium of those extracts from him, which Polyhistor had inserted in his *Historic Collections* from various authors, about 250 years after Berosus and Alexander. As Scaliger has

though Syncellus fixed this birth in the reign of Ninus, yet Africanus did not, but 200 years before Ninus, therefore Africanus was not inconsistent with himself. Syncellus ought rather to have shewn the cause of this difference between them, which was, that Africanus in fixing the exit of Moses under Ogyges, placed it 200 years *too early*, as Eusebius proves in his Preface; hence Africanus was forced to fix the birth of Abraham in *his own* year of the world 3202 (as Syncellus says p. 92) but this date was 200 years before *his own* date of Ninus, as may be easily proved, by his date of the first Olymp: in the first of Ahaz. P. 197. V. Jackson, 159.

pro-

produced no evidence for his assertion, which has so much misled all later writers, we can only conjecture at the origin of his error; which might have been because Africanus was the *last* author quoted by Syncellus, *before* he inserts his list of Assyrian kings from Belus; this is at p. 93, and his Assyrian catalogue follows at p. 96; but that quotation is concerning a fact no way relative to the kings of Assyria, and only to Jewish history, namely, with respect to the date, at which Abraham entered the land of Canaan.\* This date Syncellus disapproves, and he proceeds to refute it in the subsequent pages as far as p. 96, where he quits that *scriptural* subject, and makes a sudden transition, without any prefatory information, to three catalogues of *profane* kings; 1. Of kings of Egypt or Mestram. 2. Of a different succession of kings at Thebes in Egypt. 3. Of Assyrian kings at Babylon, beginning with Belus. Now it is possible, that Scaliger might refer all these three profane catalogues to the same last-quoted authority of Africanus; but a little more attention to the arrangement and method of Syncellus,

\* " *Ex Africano. Ann. Mund. 3277. Abraham terram Canaan ingressus est.*" P. 93.

would

would have shewn the impropriety of such a supposition by Scaliger; for not any one of those three catalogues of profane kings *begin* at the above-mentioned p. 96, they being only *continuations* of three catalogues, which Syncellus had *begun* long before, but broken off several times, agreeably to his usual method of giving first a certain *portion* of *profane* history, and then an account of such *Jewish*, as was cotemporary with it: so that the *portion* of Assyrian history inserted at p. 96, and beginning with Belus, has not the least connexion with, or any reference whatever to any authorities quoted before concerning any portion of Jewish history; but each portion depends for its computations upon its own separate authorities. Now what the authorities were, from which Syncellus derived this Assyrian catalogue at p. 96, he does not indeed there inform us, but introduces that *portion* rather abruptly, without any prefatory information concerning it; the reason, however of this conduct, we may discover without much difficulty, by looking backward to what he had said before: for this *Assyrian portion* at Babylon being only a *continuation* of a succession of kings there, which he had begun and broken off again by *portions* se-



veral times before, we are naturally led back to the *very beginning* of that succession for the requisite information concerning it; and must therefore go back as far as to p. 14, where he *first* introduces his account of the succession of antient kings at Babylon; and where he also informs us of his authorities for it, namely, those of Berosus, Polyhistor, Abydenus, and other profane historians with respect to Babylon, and that of Manetho in regard to Egypt.

It seems as wonderful then, that all modern Chronologers should have so blindly followed Scaliger in attributing the Assyrian catalogue of kings to Africanus, because he was accidentally quoted not long before concerning Jewish history; as that they should not have observed for themselves, that all the accounts of Syncellus were derived by him from those profane authorities, which he himself had mentioned at first; and which are afterwards repeatedly quoted for them all along from p. 14 down to p. 51, where he first begins his account of Egyptian kings from Manetho, comparing these as he proceeds with the accounts of Eusebius as well as of Africanus: this Egyptian account he finishes at p. 78, but expressly with this farther information, that as the *preceding* catalogue  
of

of Egyptian kings was taken from Manetho, Africanus and Eusebius, so his *subsequent resumption* of the succession of kings at Babylon will be copied by him from *profane Greek historians*.\* Now who these profane historians were, he immediately clears up to us, by proceeding in his very first paragraph in the very same page, to quote again the very same *Greek historian* Polyhistor (so often quoted before) for this promised *subsequent* account of the kings at Babylon: and as before he had quoted Berosus and Polyhistor as his chief authorities for those Babylonian kings *who reigned before the deluge*, (as may be seen in my note to p. 137) so he now quotes the same Polyhistor for the *subsequent* Babylonian succession of kings *after the deluge* as far as to the *dispersion* of the nations at Babel, when *Evechous* began to reign; who, as he pretends, was the same as Nimrod in the Jewish scriptures, and who was succeeded by six other Chaldean kings at Babylon.

\* “ Huc usque Manetho, quæ sequuntur ex Græcis scriptoribus depromuntur.” Εξ ελληνικῶν συγγραφεῶν. I believe that ελληνικῶν always means *profane Greeks*; but that at least it does so here, appears by his immediately after quoting Polyhistor, and then distinguishing him from *Christian writers*, by calling the latter, in the very next sentence, ἐκκλησιαστικῶν ιστορικῶν.

That these seven Chaldean kings at Babylon *after the dispersion* were in like manner copied from Polyhistor, is sufficiently proved at the above p. 78, where he expressly says, that Polyhistor had given such an account of seven Chaldean kings there, beginning with *Evechous* or Nimrod; which Syncellus himself deems (as he adds) to have been the first *real* kings, who ever existed at Babylon, all preceding ones mentioned by Berosus and Polyhistor being mere fables, as he conceives.\*

From the words last quoted we find, that Syncellus promises, after giving some account of *the Jews*, to *resume* his account of the succession of *real* kings reigning at Babylon, *after the dispersion* of nations in

\* “ Polyhistor Chaldæorum regnum *post Diluvium* continuata serie rursus propagatum fabulatus est, et regnasse 86 reges Chaldæorum et Medorum annis 30,000.—Ab 86 præfatorum regum tempore *alteram seriem* Chaldæorum regum nempe sub Evechoum et 7 reges imperantes annis 190, legitima solarium annorum ratione habitâ, *Polyhistor idem inducit*—Ab hinc initio ducto [i. e. post dispersionem] Chaldæorum regnum, cui primus Evechous seu Nimrod præfuit, Ægyptiorumque pariter princeps fuit Mestraim *nos Asserimus* incepisse.—Necessario itaque reliquum est ut ab annis mundi 2776 [i. e. a *dispersione gentium*] cum illuc usque sermonem antea produximus, narratio nostra de *duobus* illis regnis nunc quoque ordine procedat.” P. 78 & 79.



the same regular method as he had already done of those *fabulous* ones, from the *creation* to the *deluge*, and from the *deluge* to the *dispersion*; at the same time he promises to give an account likewise of Egyptian kings, the first of whom, Mestrim, he conceives to have begun in Egypt at the same time with Evechous or Nimrod at Babylon, both soon after the *dispersion*. This Egyptian succession he begins at p. 91, and professedly from Manetho; but as to that of Babylon at p. 90, he does not expressly mention from what author he copies it; yet after the above words produced from him at p. 78, in which he declares *in general*, that he *shall* copy his *succeeding* accounts from *profane Greeks*; and accordingly does immediately after quote Polyhistor as one of these, and says, that Polyhistor did give such a catalogue of seven Chaldean kings *after the dispersion*, the first of whom was the same Evechous or Nimrod, and the second named Chosmabelus \*, both of them exactly as in his own catalogue at p. 90; there can be no room to doubt, but that this catalogue of Syncellus at p. 90, was copied from that profane author. His promise moreover, that

\* Εὐνηχοῦ καὶ Χοσμάβηλου. P. 78.

his

his *succeeding* accounts should be taken from *profane Greeks*, must extend not only to this first *portion* of the kings at Babylon *after the dispersion*, but in general to all the remaining portions *after this*; and consequently to his catalogue of Assyrian kings at Babylon, beginning with Belus at p. 96; which is the *last portion* of the succession in question. It is to be observed likewise, that all these *portions* after the dispersion, containing *three* different races of Babylonian kings, so regularly succeed each other, that each *subsequent* one *begins* precisely in the very next year of the world after the *preceding* one *ends*. Who then can doubt, but that they were all copied from the same profane author? And as the first race of kings was certainly taken from Polyhistor, so therefore must the two remaining ones be.\* But in all these Babylonian accounts there is not the least mention of

\* “ Floruit post dispersionem anno mundi 2775, Chaldaeorum regum 7 imperium annis 225; desinit anno 3000. A 3001 Chaldaeorum imperium obtinuerunt 6 Arabes, annis 215, usque ad 3215, (p. 96.) — Arabas exceperunt in imperio reges Assyriorum 41 ab anno 3216, usque ad 4675; annis 1460, a primo eorum Belo usque ad Mascocolerum, qui et Sardanapalus, prout plerique nobiles historici Polybius, Diodorus, Cephalion, Castor, Thallus et alii consentiunt.” P. 92.

Africanus;

Africanus; who is only quoted when Syncellus treats of Egyptian and Jewish history in the intervals between the above several *portions*, from near the *creation* down to the end of the Assyrian kingdom. What pretence then could Scaliger and others have to ascribe these catalogues of kings at Babylon to Africanus, who is never quoted as the author of them; instead of Berosus and Polyhistor, who are so repeatedly throughout referred to as the authorities from whence the first, and several of the others *in particular*, and all *in general*, are expressly said by Syncellus himself to have been derived?\*

\* “ Assyriorum regnum annis 1460 stetit—Assyriorum regnum annis 1460 stabilitum usque ad annum mundi 4675, Sardanapali 15 (16) anno, qui fuit Assyriorum rex 41, et a nonnullis Thonus-Concolerus vocatus, eversum est.” P. 165. Vignoles objects here, that although Syncellus says the kingdom lasted 1460 years, yet the reigns assigned to the kings, if summed up, amount only to 1459. But this is a mistake of Vignoles, founded on another mistake of the Latin translator: for the Greek text says, that Sardanapalus perished in his 20th year [*κ' ετη*] the translator perceiving this to be MS. error, changed 20 into 15, whereas it ought to have been 16. For Syncellus says, p. 160, that the 40th king reigned 42 years, from 4618 inclusively; add to 4617 the sum 42, they end with 4659, to which, on adding 6, they end with 4665 as above; therefore the whole reigns really amount to 1460 complete.

It



It may seem however extraordinary, that although in the quotation from p. 92, Syncellus quotes so many *profane Greeks*, he should not have included the names of Berofus and Polyhiflor among them.\* This excites a doubt, whether the Polybius there mentioned may not be a MS. error for Polyhiflor, and whether the same error does not occur again at p. 196. For certain it is, that we have no information preserved, of Polybius having been either a chronologer or historic antiquary; nor that there was ever more than one person of that name: he wrote no other history than that of the Romans, and such events of other nations as were connected with that subject; neither did he begin this at any earlier time, than where the Roman history

\* It may be farther observed, that in the above quotations Syncellus takes no notice of 1460 being a remarkable astronomic period, which was also applied to the fabulous purposes of astrology; but it appears from another passage, that he was not ignorant of this, although indeed he has not expressed any suspicion, whether this Assyrian period of 1460 might not be as fabulous as the Egyptian period of 36525, formed out of 1461 by 25. "Summa regnorum apud Egyptios est 36525, i. e. 1461 per 25 multiplicati, qui *decantatam* indicant Grecorum et Egyptiorum *fabulis* Zodiaci in pristinum statum reparationem, prout in Mercurii Genicis & Cyranibus libris continetur." P. 52.

of Timæus ended at the 1st Punic war. It is the first five books of this work, which are still extant, in which no mention is made of any history prior to that of the Persian empire; he wrote indeed a tract relative to Geography, which is often quoted by Strabo, but nothing else: so that it is difficult to conceive what occasion he could have in either work to treat of the duration of the Assyrian kingdom, or as mentioned at p. 196, of the number of Olympiads which preceded the present earliest date of them; both of which would naturally be included in the *historic collections* by Polyhistor. However, it is probable, that the historians here referred to by Syncellus, might not be quoted by him to confirm what was the duration of the Assyrian kingdom, or what it was not, but merely to ascertain the very last fact mentioned in that sentence, namely, that *Mafcoclerus* was the same Assyrian king as is called by others *Sardanapalus*: it is at least certain, that those other historians could not have been quoted in order to confirm the period of 1460 years, because Diodorus is one among them, who, as is well known, reckoned no more than 1400 years and little more; and Castor, as Syncellus informs us, reckoned only 1280, but Ceph-

lion only about 1000 (p. 167.) If then it was for the other purpose, that those historians are referred to, we need not wonder that Berosus and Polyhistor were not included, as the Assyrian name with them was on the contrary, Mascocolerus, or Thonus-Concolerus, as it is expressed in p. 165; to which name by those authors it is, that the Sardanapalus of the others must be opposed. If this was the meaning of Syncellus, the names of Berosus and Polyhistor could not be included along with those others; and Polybius is quoted for nothing more than the mere name of Sardanapalus, of which he possibly might have found occasion to introduce the mention somewhere in his history or geography.

It may be added here farther, that the late M. Barthelemy, in his *Travels of Anacharsis*, has revived the exploded date of 506 years nearly as in Herodotus, for the date of the capture of Troy before the Olympiads; as has also Larcher that of 494, in the life of Homer, falsely ascribed to Herodotus: this is neither any instance of the judgment of the former, nor of the accuracy of the latter author; but it shews the propriety of our enquiring thus minutely concerning these dates, if readers would wish to come to some fixed principles  
in



in Chronology, instead of being eternally shifted about with the fancies of every new writer. The error of Herodotus concerning 508, was however only the common error of his age, but the reputed fact mentioned by him of Ninus being descended from Hercules is of this advantage to ourselves, that it proves the same common error of 100 years too early to have been attributed in that age to the date of Ninus as well as of Troy; so that the 520 years of the former ought to be reduced to 420, as well as the 508 of the latter to 407. This then accounts for the otherwise unaccountable fact, that Homer should have made no mention of the existence of an Assyrian kingdom at the time of the capture of Troy; for it thus appears, that the conquests of Ninus were *posterior* to that capture. This date is farther confirmed by the Asiatic tradition, that Memnon, king of Ethiopia, was co-temporary with and present at the siege of Troy; and both Diodorus and Pausanias mention, that Memnon had extended his conquests over Syria and even Persia.\* Ovid says, that he was slain

\* "Memnon venit ad bellum Trojanum non ex Æthiopia, sed a Sufis Persarum urbe, debellatis omnibus nationibus usque ad Choaspem flumen." *Paul. lib. 10.*

by Achilles, (Metam. xiii. 580.) which is confirmed by Hyginus (Fabul. 112.) But as Homer says nothing about Memnon, these must be all Asiatic traditions, as indeed Pausanias sufficiently suggests along with Diodorus, when the former says, "Monstrant *Phryges* quo itinere exercitum duxerit Memnon." Now the age of Memnon was actually consonant with the siege of Troy; for he was the son of Tithonus, the son of Laomedon king of Troy, whom Hercules had slain in the foregoing century. But such pretendedly extensive conquests by Memnon, so far as to Persia, shew, that the Asiatics had then no knowledge of any Assyrian kingdom existing so early as in the age of Memnon and the siege of Troy; it being impossible for him to have arrived at Persia without passing through Assyria or its borders. The addition therefore which Diodorus makes to these traditions, relative to a Teutamius king of Assyria deputing Memnon to Troy, can have been only invented in later times by the inhabitants of one part of Asia; in order to reconcile, as well as they could, that *popular* historic tradition concerning the presence of Memnon, which might have some truth at the bottom, to the chronologic fable invented by another part of Asia, concerning the anti-

quity

quity of the Assyrian kingdom, which had no other foundation than the imposition of *astrologers*. This popular tradition however concerning Memnon, if it be entitled to any credit, tends to confirm the computations of Herodotus; which fix the existence and conquests of Ninus *later* than the Trojan war, as in like manner does the silence of Homer concerning any Assyrian king having been concerned in sending assistance to Troy.

But possibly it may be still objected, that although the period of 1460 was not copied from Africanus, yet it might have been taken from some *later* historians, since Syncellus only says, that it was copied from *profane Greeks*, without mentioning who they were; therefore Cumberland may still be right in calling the adherents to 1300, the *elder* authors. Now even if there could be any room to doubt, whether one of those *profane Greeks* in question was Polyhistor, 300 years before Africanus, yet Diodorus sufficiently proves, that a period not less than 1460 had been received before his own age under Augustus, 200 years at least before Africanus. For he says, that either Ctesias or some other writers assigned *more than* 1400 from Ninus: but as Belus has never less than 65 or 60, or 55 years allowed



allowed to him, it is evident, that the writer in question could not have reckoned less than 1460 from the accession of Belus, and this 200 years before any of those authors lived, who adopted only 1300; this it is therefore which makes Cumberland and others so desirous to alter the text of Diodorus, and thereby to set aside his testimony to the antiquity and priority of 1460 before 1300. This consistency of the testimony of Ctesias and Diodorus, with that of Berofus and Polyhistor, shall be considered farther in my next section, as this again indisputably proves the antiquity and originality of the period of 1460, notwithstanding all the endeavors of Cumberland either to find or to make Ctesias to be at variance both with himself and Syncellus. Here therefore I cannot but apply a complaint of M. Dupuy, "that even concerning subjects which have been repeatedly treated of by a variety of authors, it often happens nevertheless, that in order to arrive at truth, it becomes necessary to begin with laying aside every thing which has been already said by the learned, that we may search out for ourselves the true state of every case, without being any way biased by the opinions, errors, or misrepresentations of others."\*

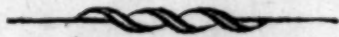
\* Journal des Sçavans, pour Janvier 1789, p. 29.

CRITICAL OBSERVATIONS  
ON  
BOOKS,  
ANTIEN<sup>T</sup> AND MODERN.  
NUMBER XII.

TO BE CONTINUED OCCASIONALLY.

CONTAINING

Proofs from Ctesias and Diodorus, that 1460 was known to the Greeks as the Period of the Assyrian Kingdom some Centuries before Africanus—The Text of Diodorus vindicated against the Variations proposed by Petavius, Marsham, Cumberland and Jackson—Internal Evidence that the Period of 1460 was reduced to 1300 by the Greeks in order to suit with their similar reduction of 160 Years from the Antiquity of Troy—Statement of their different Computations of that Capture in different Ages—A Table of the Dates of Events from the End of the Assyrian Kingdom to the End of the Median Kingdom, which succeeded, proving the Assyrian Kings in Scripture to be the same Persons as the Kings of the Medes in Ctesias agreeably to the Opinion of Prideaux.



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# CRITICAL OBSERVATIONS

ON

## BOOKS

### ANTIENT AND MODERN

#### NUMBER XII

##### ERRATA.

- P. 73, in note, for sect. 46 *r.* 47.  
 P. 109, lin. 3, for Agron *r.* Alcaeus.  
 P. 132, lin. penult, for Belus *r.* Ninus.  
 P. 207, *dele* Herodotus.  
 P. 274, to the name *Syncellus*, add p. 185, 195.

##### ILLUSTRATIONS.

- P. 133, lin. 10, for Tiberius *r.* Augustus.

✂ The Assyrian computation communicated by Calisthenes shall be included among the *Illustrations*, proving it to have commenced as by Ctesias 1360 years before the end of the kingdom.

LONDON

And sold by R. WHITE, 11, St. Paul's Church-Yard.



*The Antiquity of the Periods 1460, 1400 and 1360, confirmed by Ctesias, Diodorus and Calisthenes.—Objections of the Moderns removed in regard to the Sums 1400 and 1360 in the present Text of Diodorus, particularly the objection urged by Petavius, and adopted by Marsham, Cumberland and Jackson, that Syncellus mentions 1300, instead of 1400, as being the Sum, which he found in his own Copy of Diodorus.*

HAVING proved the *antiquity* and *originality* of the period 1460 for the Assyrian kingdom, by its having been communicated to the Greeks in the history of Asia by Berosus, himself a Chaldæan priest at Babylon and 500 years before Africanus, extracted thence by Polyhistor above 200 years before Africanus, and copied directly from one or other of those two profane authors by Syncellus in preference to the sum of 1300 in Africanus and Eusebius; it may nevertheless be still more convincing, if I confirm the antiquity of these sums still farther, by shewing, that such longer sums than 1300 were at least *known* to the Greeks *before* the age of Berosus by the information of Ctesias

and Calisthenes ; and moreover *after* Berosus are mentioned by Diodorus as computations then current two centuries before Africanus : so that even if the fact were true, that 1460 was the sum adopted by Africanus, still it must have been derived by him from more antient authors, and not have been the invention of himself or any other christian author of so late an age, as some moderns seem to suspect.

Now Diodorus has in one place, expressly said, “ that Ctesias himself (almost a century *before* Berosus) mentions *more than* 1360 years, in his second book, as being the duration of the Assyrian kingdom :” and in a *second* passage afterwards, Diodorus says again, but without quoting any particular authority, “ that *more than* 1400 years were commonly assigned to that kingdom, although reckoned from no earlier event than the accession of Ninus.” \* This *second* computation not being expressly

\* Παραπλησιον δε τουτω [Νινου] και οι λοιποι βασιλεις παις παρα πατρος διαδεχομενος την αρχην επι γενεας τριακοντα εβασιλευσαν μεχρι Σαρδαναπαλου επι τουτου γαρ η των Ασσυριων ηγεμονια ΜΕΤΕΠΕΣΕΝ εις Μηδους, ετη διαμεινασα πλειω των χιλίων και τριακοσίων ετι δε εξηκοντα, καθαπερ φησι Κτησιος ο κνιδιος εν δευτερα βιβλω,

Η μεν ηγεμονια των Ασσυριων απο Νινου διαμεινασα μεν τριακοντα γενεας, ετη δε πλειω των χιλίων και τετρακοσίων υπερ Μηδων ΚΑΤΕΛΥΘΗ.

pressly ascribed to Ctesias, as the *first* is, it may therefore have been taken from Berosus or some other Greek historian; but from whomsoever it was taken, it proves, that the original author could not have reckoned much less than 1460 years from Belus: and indeed if we suppose, that *more than* 1400 means 1405, the addition of 55 commonly assigned to Belus, makes the sum amount exactly to the whole period 1460. These longer sums thus antiently testified to both by Ctesias and Diodorus stood so directly in the way of those moderns, who had a predilection for the shorter sum of 1300, that they have been forced to try every possible means to invalidate an evidence which so plainly contradicted their system: let us then examine what objections they have been able to invent to remove this obstacle to their own opinion. The *first* objection has been by affirming without proof, that the *second* sum 1400 is taken from Ctesias as well as the *first*, therefore as these sums are (they say) in contradiction, we can place no dependence on either of them. But this objection assumes what is not true, that Diodorus takes every article of information from Ctesias; for, on the contrary, he often tells us, that other authors disa-



gree with Ctesias in such or such facts, and this *second* sum 1400 may have been intended as a farther example of such disagreements: so that it is no proof of contradiction in Ctesias himself, that Diodorus quotes two different sums from two different historians\*. Nay, if it were ever so certain, that both of those sums 1360 and 1400 were taken from Ctesias, yet still there would be no sufficient proof of any contradiction, because it is not certain that those sums were both reckoned down from the same event; the *second* sum 1400 is expressly said to be reckoned from Ninus; but in regard to the *first* sum 1360, this expression of Diodorus, *the dominion of the Assyrians*, is so very vague and indeterminate, that it cannot with certainty be concluded from this phrase whether he meant to reckon this sum from Belus or from Ninus, or from Ninyas, or any other event: but if reckoned from the time when Ninus had finished his chief conquests in Asia about

\* “ Hæc quidem Ctesias, at Athenæus et alii scriptores asserunt sic,” &c.—Hæc sunt quæ *contra-versa* de Semiramide narrant auctores.—Altitudo mænium Babylonis omnium, ut Ctesias scribit, excederet; ut véro Clitarchus et qui cum Alexandro literis prodiderunt sic—Quinquaginta orygas, teste Ctesia, altum, vel ut alii referunt, quinquaginta cubitos,” &c.

about the 46th year of his reign, at which time, and not before, *the dominion of the the Assyrians over Asia* could alone with propriety be said to commence; and at which time he had completed his new city of Nineveh, there would then be exactly 1360 years from this event to the end of the kingdom according to Berosus. Now it is *possible*, that this might be what Ctesias *himself* means by *ἡγεμονία τῶν Ἀσσυρίων*, namely, not *the monarchy of Ninus over the Assyrians*, but *the dominion of the Assyrians over Asia*. It is however true, that this, although the proper one, is not the sense in which Diodorus employs the phrase; for, in the *second* of these passages in question, he reckons this *ἡγεμονία* expressly from the *accession* of Ninus, and in *both* of them he reckons up the same number of generations, namely, 30: therefore, both must be reckoned from Ninus by him, though not possibly by Ctesias himself, whose own words there quoted, seem to be intermixed with and interpreted by the addition of others by Diodorus; so that we cannot determine whether any thing more than the mere sum itself strictly belongs to Ctesias. In a *third* sentence, however, it might at first be presumed, that Diodorus employs the phrase in a more  
proper

proper sense, yet in reality he does not, but in this case is more deficient in propriety than in the *two* preceding cases: for he says in this *third*, "that Troy was captured when the Assyrians had possessed dominion over Asia more than 1000 years\*." Here we find the words *over Asia* expressly inserted; if then he had adhered to strict propriety, he could not in this *third* sentence have computed those 1000 years from any earlier event than the *conquest of Asia*, which was not effected until near the *end* of Ninus: yet the real fact seems to be, that those 1000 years are reckoned from the *accession* of Ninus at least, if not from that of Belus. At what year of his reign Ninus began his conquests is not mentioned by Diodorus, but he says, that he was employed in them 17 years, and that after having completed them, he built Niniveh as a lasting memorial of his success: the erection of that city must then have been near the close of his reign, as he undertook only one expedition afterwards, namely, against Bactria, in which he perished. Agreeably to this, Eusebius fixes the erection of Niniveh in the 43d of Ninus;

\* Πλείων των α ετών οι Ασσυριοι την ηγεμονίαν της Ασίας έχοντες. Lib. 2.



rus; but he might mean the first foundation of it, and it might not have been finished until the end of his 45th year, exactly 100 years after the beginning of the kingdom by Belus. From the 46th year, therefore, and not sooner, those 1000 years ought in strictness to be computed, if they began with the *dominion of the Assyrians over Asia*, as the words of Diodorus affirm in this *third* sentence: but if, instead of attending to the *phrase* only, we examine the *sum* itself, we must draw a different conclusion; and one which proves, that the words *over Asia* give no assistance to fix his sense of *the dominion of the Assyrians*, by determining at what event he conceived that *dominion* to begin, whether at the accession of Belus or of Ninus, or at his completion of the *conquest of Asia* in his 45th year. For all the antient accounts consider the Trojan war as happening under the 27th king *Tautanes*; but there were not quite 1000 years from Belus expired even at the end of the 28th king, *Teutæus*: if *Tautanes* then, or his successor, be, as they suppose, the same as the *Teutamus* of Diodorus cotemporary with the Trojan war, it follows, that those 1000 years could not be reckoned from any later date than the *accession* of Belus. This *sum*  
of

of 1000 years before Troy proves still farther, that the original author of this computation, whoever he was, had *before* the age of Diodorus, allowed 1460 years for the sum total of the kingdom: for after deducting the above 1000, there remain only 460 to the end of the kingdom; and 91 more thence to the Olympiads, amounting to 551, which was not more than the *earliest* Greeks allowed to the end of the interval between Troy and the first Olympiad. Those of the *middle* age indeed, Herodotus and others, had reduced that interval to 508 or 518; yet these must still have assigned more than 1400 at least to the whole period: for even thus, after the 1000 years before Troy are deducted, there would remain only 400 after it; which added to 91 after the end of the kingdom, would amount only to 491 for the interval from Troy to the first Olympiad, instead of 518. That sum of 491 was accordingly nearly agreeable to the computation in the life of Homer, commonly ascribed to Herodotus, which made the interval 494; so that this last interval proves again, that the author, whoever he was, had allowed *not less* than 1400 years to the whole period: and he must even have allowed 1460, in case he reckoned the 1000 years before Troy from

no earlier date than the accession of Ninus. But these 1000 years before Troy are rather less compatible with the supposition of those later Greeks, who assigned only 1300 years to the whole kingdom; for thus there would remain only 300 years after Troy, and 391 only to the first Olympiad, which is something less than the 407 which they usually did allow: yet still in this case those 1000 years in question must have begun as early as the *accession* of Belus; but in the second case might indeed have begun as late as the accession of Ninus, though not later. So that by every view which we can take of the *sum* of 1000 years before Troy, it proves, both that they must have been reckoned from the accession of Belus or Ninus, and also that *more* than 1300 if not 1460, must have been at first received as the whole duration of the kingdom long *before* Diodorus; for he seems to refer to that sum of 1000 years as being an antient computation, if not that of Ctesias himself. This proves, therefore, that by the words *over Asia* being added to the phrase *dominion of the Assyrians*, no alteration is made in the meaning of Diodorus, and that this *dominion* in question must, according to him, either commence with Belus or Ninus, not later; and



that the phrase itself contains no determination of the commencement of the *dominion* in question. Since then this *dominion* in the *second* sentence is expressly said to have begun with Ninus, and in the *third* as certainly must have begun with Belus, in case the antients have rightly fixed its *end* at the 27th king; or in case those are right who contend that there were only 1300 years in the whole duration; we have consequently no sufficient authority afforded us by any words of Diodorus to affirm, that the *dominion* of 1360 years in the *first* sum from Ctesias did not commence at a still different event from the other two, namely, at the completion of the *conquest of Asia* by Ninus in his 45th year: by this supposition that phrase the *dominion of the Assyrians* would be employed here with still more propriety than in the other two cases; and moreover, as these words are expressly quoted from Ctesias, this might at least be the meaning of Ctesias himself, although Diodorus, in the hurry of making his collection from different authors, has thus omitted to ascertain from what event both this *first* and also the *third* computation were reckoned by the original authors of them. For since he has certainly neglected to ascertain the commencement of the *third*  
sum

sum of 1000 before Troy, though different from that of the *second* sum 1400, he may, therefore, have just as well, through neglect, have omitted the commencement of the *first* sum 1360, though different from both the others. Hence it follows, that there may be no *contradiction* in the text of Diodorus by his assigning those different sums relative to the Assyrian *dominion*, 1000, 1400, and 1360; for they may thus differ in duration only because the *original authors* began to reckon them from different events in the Assyrian kingdom, one from Belus, one from Ninus, and one from the complete conquest of Asia and dominion over it. The commencement of this last computation has at least this farther circumstance to confirm it, namely, that there were in fact exactly those 1360 years from the completion of those conquests and the erection of Nineveh at the end of the 45th year, 100 years from Belus, to the end of the Assyrian kingdom; and since Ctesias says *more than* 1360, he might possibly reckon that overplus from the foundation of Nineveh in the 43d of Ninus. That same want, then, of sufficiently explicit information by Diodorus, which at first may give the appearance of *contradiction* between the three sums mentioned by him,

does at the same time render the foundation of such an accusation too insecure to correct his text upon that presumption; especially since they would become all harmonious, in case they were reckoned from three different events, and thus be only different *portions* of that sum of 1460 years contained in the *whole duration* of the kingdom. Accordingly, Vignoles has anticipated me in thus removing the objection of contradiction between the *sums* in the present text of Diodorus, upon the same plan of their being only different *parts* of one and the same *whole*; independently of that other consideration, that all the three sums may be taken from three different authors, who did not agree in their accounts.\* The above-mentioned 46th year  
of

\* "Diodorus does not seem to have always followed Ctesias, and the sum 1400 he mentions as *from himself* [rather from some other author].—It is not, however, difficult to *reconcile* that sum to 1360 in Ctesias; for if we add to these the 42, which Eusebius allows before Ninus became *Emperor of Asia*, they amount to 1402; so that Diodorus had good reason to reckon *more than* 1400 after the accession of Ninus." *Lib. 4. 191 et 193.* Again—"It is not difficult to *reconcile* the different sums of different authors in Diodorus, if we do but distinguish the *monarchy of the Assyrians* from their *empire over Asia*." *P. 210.* There is no occasion to go so far as to say there is no *difficulty* in the case;  
it



of Ninus, as being the commencement of his proper DOMINION over *Asia* might have become afterwards a common epoch of computation in that country; and accordingly I shall shew, that a computation sent from Babylon by Calisthenes, who accompanied Alexander, begins at that very date: yet it is nevertheless very possible, that in time they might have forgot the event, from which that computation of 1360 commenced; and might have erroneously conceived, that it began with the accession of Belus or Ninus, although in reality it did not commence until the completion of his conquests in Asia, and the erection of Nineveh. We have a similar example in regard to the æra of Alexander, which is still a common mode of computation throughout Asia; and yet they have now forgot its origin; for they conceive it to have commenced at the death of Alexander the Great, although in fact it does not begin until the death of his son  
 Alexan-

it is sufficient to point out, that *the dominion of the Assyrians* may with as much or more propriety be dated from the conquest of *Asia*, than from the accessions of Belus or Ninus; and that the 1360 may be so dated by Ctesias in that first sum, although other authors dated that dominion from Belus or Ninus.

Alexander Ægus, 12 years later, as I have already shewn. It might have been unnecessary to have considered this insufficient objection so minutely, if it had not been rather done in order to give a true statement of the *facts* concerning the several sums in question; which other authors will be found to have often misrepresented, and to which I may afterwards have occasion to refer.

To this objection, under pretence of *contradiction*, Petavius was the first to suggest some others founded on the supposition of *corruptions* in the present Greek text; which, however, are such as only prove what unsolid evidence even able writers are not ashamed to employ, in order to prop up an arbitrary system of opinions: for it was necessary to his prepossession in favour of the later date of the deluge in the Hebrew text, to maintain that the true sum of the Assyrian kingdom did not exceed 1300 years, in order that it might not commence so early as the deluge. But it becomes requisite that these objections should be removed, as other able authors, such as Marsham, Cumberland, and Jackson, have, by implicitly adopting them, seemed to allow their validity; and as they have attempted scarcely any additions or improvement

ment upon the evidence adduced by Petavius, we must therefore go back to him, as the original source of their errors. Now so long as the period of 1460 years in Syncellus was supported by no earlier testimony than that of Africanus, or Syncellus himself, it might be an easier task for Petavius to bear down such late authorities by the weight of plausible conjectures: but the evidence of such an antient and reputable historian as Diodorus, collected out of still more antient profane authors, before christianity had taken the Hebrew text under its patronage, requires some stronger proofs to remove it, than either mere suppositions of manuscript errors in that profane author, or confident imputations of error and ignorance in those christian ones; let us see then what evidence he has been able to collect to oppose to either of them.\*

His

\*“ Assyriorum imperium Diodorus e Ctesia ait annos perseverasse 1360; sed perperam ab aliquo addita fuisse probabile est illa verba *ετι δε εχρηματα*, neque enim satis apta est series orationis; quis non videt ab imperita manu esse hanc appendicem ejus qui excurrentem numerum importune concepit aut ad æram adnotavit, unde postea in contextum a librariis inculcatus est? In fragmentis Syncelli a Scaligero collectis (*P. 30 vel 32.*) ex Diodoro non anni 1400 sed *ατ* (1300) numerantur. Quinetiam Clemens



His first objection is to the *first sum* 1360, which Diodorus ascribes expressly to Ctesias, 400 years before his own age, and 600 before Africanus: against this Petavius alleges, that the last 60 years have much the appearance of an *interpolation* in the Greek text. But by such arbitrary conjectures as these, we may expel any words we please from antient authors which oppose our own opinion; and the uniform consent of all the manuscripts of Diodorus to this *first sum* 1360 is of itself a sufficient answer to such an unnecessary alteration of the Greek text. As to the pretence, that the words of the sentence are not aptly connected, who can take upon him to affirm, that Diodorus, a Sicilian, and in a late age of Greek literature, always disposed his Greek words in the most approved order of construction? It might just as well be asserted, that Petavius could  
not

mens Alex. (Strom. 1) totidem annos ex Diodoro et Ctesiâ tribuit; quibus etiam auctoribus constat 26<sup>m</sup> a Nino fuisse Teutanem, qui Trojanis auxilia submit, cum jam supra annos 1000 Asiæ dominarentur Assyrii.—Justinus Assyrios 1300 annis imperâsse prodit, atque ita Ctesiam et Diodorum scripsisse verisimile est.—Quocirca nos cum Eusebio, Justino et Augustino (12 *de civ. c.* 10), ac plerisque veterum Assyriorum regnum durâsse credimus annos præter propter 1300 a Belo.”—*Doct. temp.* 9. 13.

not have writ such Latin as is found in this accusation, his censure being of the *whole words*.

His next objection is directed against the sum 1400 in the *second* passage of Diodorus; and he contends, that Syncellus, by a quotation which he makes of that sentence, proves that the manuscripts made use of by him had only 1300, not 1400; which is confirmed, he adds, by Clemens, who quotes, he says, Diodorus and Ctesias, as assigning 1300 years only to the Assyrian kingdom. But the real fact is, that Clemens never quotes those authors at all concerning this sum; but only as to what the Assyrian reign was, in which Moses made his exit from Egypt, and even this is not quite clear in the present corrupt text: but if it was ever so clear, yet it is impossible to collect from this fact any knowledge of the total duration. This whole objection then rests upon the following circumstance, whether the words of Syncellus sufficiently testify, that he found in his own copy of Diodorus 1300 years *instead* of 1400, as Petavius supposes him to have done; but of which we shall find no satisfactory evidence in his own words: for if Petavius had faithfully represented them, it would have appeared, that Syncellus in reality quotes Diodorus as assign-

ing *more than 1300 years* to the Assyrian kingdom, *instead of more than the 1360* in the *first* sentence of Diodorus, not *instead* of the 1400 in the *second* sentence; his words being precisely the very words of Diodorus in that *first* sentence, which prove therefore, that Syncellus had only that *first* sentence in his view and meaning.\* So that if it were even true, that the omission here of *ἐτι δε ἐξηκοντα* proves these words to be an interpolation in our present text of Diodorus, yet it does not follow hence, that there is any error in regard to the 1400 in the *second* sentence, of which Syncellus takes no notice at all; and thus its testimony still remains fully in favour of so long a *sum* having been assigned by some antient author or other *before* the age of

\* *Παρτεπλησιως δε οι λοιποι βασιλεις παιδες παρα πατρος διαδεχομενοι την αρχην εβασιλευσαν επι γενεας ME μεχρι Σαρδαναπαλου. επι τουτου γαρ η των Ασσυριων ηγεμονια ΜΕΤΕΠΙΕΣΕΝ εις Μηδους ετη διαμεινασα πλειων α και τ καταπερ φησι Κτησιος ο Κνιδιος εν τη β βιβλω.*—P. 166. Compare these words with the *first* sentence of Diodorus at my p. 164, particularly with the word *μετεπεσεν* in one sentence, and *κατελυθη* in the other, and also with the order of the words and the reference to Ctesias. As to his omission of *ἐτι δε ἐξηκοντα* it may be accounted for by his meaning to reckon only from Ninus just before mentioned, as we shall see by his addition of this name to his *repetitions* of the sentence afterwards.



of that historian. But, in fact, it is not clear, whether the same integrity may not be also affirmed of the *first* sentence; for the words of Syncellus are *more than* 1300, and certainly 1360 are more than 1300; how then can we know, whether Syncellus meant 1360 or any less sum, and whether the omission of *ἐξῆκοσται* arose or not from brevity and an indifference as to the exact sum above mentioned? for any way it was much less than what Syncellus considered as the *true* sum, 1460.\* However, if that omission by him should be still thought to contain some doubtful evidence of *interpolation* in the text of Diodorus, yet it certainly contains no evidence that Syncellus had found 1300 instead of 1400 in his own copy.

Nevertheless, Marsham has made a slight attempt to support this evidence against the integrity of the text in Diodorus produced by Petavius; and although

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\* Marsham therefore has been here less faithful than Petavius, for he quotes Syncellus as saying 1300 *aut paulo plures*; but there is no word in the Greek to restrain the sum to the sense of *paulo*. Yet Vitringa has carelessly copied the addition of *paulo* by Marsham. Vid. his *Hypotyposis Chron. Sacr.* P. 66. Why then may not Syncellus have quoted Diodorus as negligently as these three learned moderns have quoted Syncellus?

the opinion of Marsham himself was in favour of the 520 years of Herodotus, yet he has joined in taking up the staff to oppose the present text of Diodorus: but he seems to have thought the conjecture of *interpolation* to be too bold a correction, he therefore changed it to that of a *manuscript error* in ἐξηκοντα being inserted instead of εἰ; and this he produces Agathias to confirm, who says, “that Diodorus, on the authority of Ctesias, relates, that the Assyrian kingdom was destroyed after it had continued a few years more than 1306 from the first supremacy of Ninus there.” \* Hence Marsham concludes, that Agathias had found 1306 years as the sum of Ctesias in Diodorus, *instead of* 1360, not *instead of* 1400. But neither is this conclusion any more tenable than those of Petavius; for how do we know, whether the error, if there really be one, is not rather in the manuscript of Agathias, than in those of Diodorus? Why should all manuscript errors be adjudged to the original rather than to the

\* Εξ τε και τριακοσιων ηδη προς τοις χιλιοις και ολιγω πλειονων ετων παρωχηκοτων αφ' ου τα πρωτα ο Νινος των εκεινη κατεσχε πραγματατων. Assyriorum regnum destructum fuisse paulo pluribus quam 1306 annis elapsis, ex quo Ninus ibi primum summam rerum obtinuerat.—*Lib. 2. p. 63.*

the copies? Vignoles does at least suggest a plausible reason to prove, that the error is rather in Agathias, "for," says he, "is it possible, that any historian could think of informing readers that a kingdom lasted 1306 years and a *few* years more?" Whereas, 1360 years and few more would have been a rational expression. However, we shall see afterwards, that there is no sufficient reason to suspect any manuscript error either in Diodorus or Agathias, for that the sums are reckoned from two different events, the 1360 in Diodorus not from Ninus, but, 1306 in Agathias expressly from Ninus only; and as to *καὶ*, it may have crept in instead of *ἢ*, owing to the contractions in manuscripts, which are so various, so unsteady, and so abstruse, that it is impossible to be always secure of their meaning. Marsham, indeed, attempts still farther to support his conjecture by Augustine's having 1305 years: but this is still a worse pretence, for Augustine reckoned his 1305 expressly from Belus, whereas the 1306 of Agathias were from Ninus only\*; in which Augustine copied from Eusebius, not from Diodorus, Eusebius being the only other author extant  
who

\* De Civit. Dei 18. 20. and 12. 10.



who made so few as 1305 from Belus. One evidence, however, may be suggested in favour of the 1306 of Agathias, which Marsham has omitted; for Syncellus says, at p. 359, "that Diodorus after Ctesias assigned 1306 years to the Assyrian kingdom." \* And from the subject there in question concerning the Persians, as in Agathias; and also by the mention made there of Bion and Polyhistor, as again by Agathias in like manner, it is not improbable, that Syncellus copied these words from Agathias, who lived 200 years before him: but although Syncellus, through brevity, says nothing here of those 1306 years being reckoned from no earlier king than Ninus, yet from Agathias himself we know this to be the real fact; therefore that sum is no way inconsistent either with the 1360 in Diodorus, reckoned (as I shall shew) *not from Ninus*, nor yet with the *more than 1300* of Syncellus, reckoned *from Ninus*, and it proves, that Syncellus really meant

1306

\* Εβασίλευσαν οἱ Ασσυριοὶ ἐτη αἱτ', οὕτω γὰρ λέγουσι Κτησία καὶ Διοδώρος συμφθεγγεται.—"Although Eusebius and Augustine assign but about 1300 years from Belus, yet I cannot doubt but they being engaged in disputation with heathens, took their accounts of this empire from heathen historians."—Cumberland, p. 192.

1306 not 1360. This passage then in Syn-  
cellus, even though not taken from Agathias,  
confirms the present text of both authors,  
and disproves another conjecture of Petavius  
added by him afterwards, that εἰ in Aga-  
thias is a *manuscript error* for εἰκνοῦντα; which  
indeed is a direct contradiction not only of  
Marsham's conjecture, but also of the for-  
mer one in Petavius of its being an *interpo-*  
*lation* in Diodorus, for in such case it must  
have been inserted there before the age of  
Agathias, 1200 years ago.\* This shews  
again how lavish and extravagant critics  
are in their conjectural corrections of an-  
cient authors, since they scruple not to  
contradict even themselves, as well as one  
another: for not only has Petavius done  
this toward himself and the friendly as-  
sistance of Marsham; but Jackson also has  
followed his example.† Such conjectures,  
there-

\* In the posthumous edition of Petavius, by  
Hardouin, in 1703, made from a copy left as cor-  
rected by Petavius himself, this note is added, "In  
Agathiâ forte pro εἰ legendum εἰκνοῦντα."—*Petavius*.

† At p. 244, he agrees with Petavius, "that  
εἰ δε εἰκνοῦντα are plainly an interpolation." Yet, at  
p. 246, he says, like Petavius, "that 1306 in Aga-  
thias plainly proceeded from the *corrupt* sum 1360  
in Diodorus." But if 1360 were thus in the text of  
Diodorus, before the age of Agathias, what better  
proof can we have of their being genuine? No  
manuscripts are so ancient.

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in Diodorus." But if 1360 were thus in the text of  
Diodorus, before the age of Agathias, what better  
proof can we have of their being genuine? No  
manuscripts are so antient.

therefore, are like the incoherent dreams of men but half awake; and those also who present readers with only one side of a question, but hide the other, must in course as often deceive or mislead them. Amidst this chaos, since we find it so difficult to ascertain the original and real text of an author by conjecture, we ought at least to be as cautious in altering that of Diodorus as that of Agathias; and in fact on examining the actual meaning of both, as their texts now stand, we find no reason for any alteration in either of them, nor yet in what Syncellus has quoted from them; the several passages in all of them seem to be without any corruption.

For, as I noticed before, although Syncellus, in his above-mentioned *first* quotation from Diodorus, of *more than 1300 years*, has not said expressly, but only *indirectly*, in his *preceding* sentence, from what king he supposed those 1306 years to be reckoned, yet he has clearly informed us afterwards in two *repetitions* which he makes of that *first* quotation, that it was *from Ninus* only, and not from Belus; so that the above sum did not include the *whole duration* of the Assyrian kingdom according to his own conception. This computation of those 1306, as being  
merely

merely from Ninus, Cumberland also allows, who was as much inclined as Petavius and Marsham to object to the 1360 in the text of Diodorus, yet he could find nothing to add to assist them, except, "that Syncellus had *twice* quoted the same sentence of Diodorus, and yet reckoned in both no more than 1300." \* But he might have said *thrice*, viz. twice in p. 166, and once in p. 168. The *two repetitions*, however, make no alteration in the state of the evidence contained in them (they being merely brief abstracts of the *first* quotation), except that they both contain one *addition* concerning what was not made clear in the *first* quotation, namely, that the sum 1306 in question is to be reckoned *from Ninus* only in all the three examples.† But there is no such limitation from Ninus in the original words of Ctesias in Diodorus; it is only said there that those 1360 years were computed from *the dominion of the Assyrians*, and whether this meant from Belus or from Ninus is left uncertain; but

\* P. 193 and 194.

† Μονον δε, ότι πλειω των α τ ετων ο χρονος των ΑΠΟ ΝΙΝΟΥ λε βασιλειων εστιν, απεφηνατο [Διοδωρος]. P. 166.

Πλειω των α τ ειρηκοτα τον ολον της Ασσυριων αρχης χρονον και αυτον [Διοδωρον] ΑΠΟ ΝΙΝΟΥ έως Σαρδαναπαλου.—P. 168.



but in two other sentences where Diodorus has the same phrase, in one Syncellus *understands* it to mean *from Belus*, and in the other it certainly means *from Ninus*, this name been subjoined: in *this* then is an *addition* by Syncellus himself, who thus undertook to interpret and determine what was left undetermined by Diodorus. If then Syncellus understood Ctesias, in this *first* sentence referred to by the phrase *dominion of the Assyrians*, to mean, that the 1360 years were to be computed *from Belus* (as I shall shew from his own words that he did) he consequently could not possibly reckon more than 1306 *from Ninus*: so that the *addition* of this name in all the three quotations from Diodorus is entirely a *conclusion* of his own, and not a strict *quotation* of the very words, nor perhaps of the meaning of Diodorus; yet a conclusion which necessarily follows from *his own conception* that the 1360 years of Ctesias were to be computed *from Belus*\*.

Since

\* Agreeably to Syncellus, we find, that Jackson also conceived those 1360 years of Ctesias to be computed from the accession of Belus. "The Assyrian empire must have begun, according to Ctesias, when *Belus reigned*; for Ctesias does not say, it was more than 1000 years from Ninus to the Trojan war."—P. 244. In fact, neither does he say, "it was more than 1000 years from Belus to the Trojan war;"

Since then it is certain, that Syncellus himself has here *added* those words, *from Ninus*; which are not in Diodorus, it became necessary for him to *alter* at the same time the sum 1360 and reduce it to 1306 in order to accommodate the sum to the above *addition*. Now whether Syncellus was right or not in conceiving that the *dominion of the Assyrians* in that passage of Ctesias concerning 1360 years meant the accession of Belus is quite a different question; the only material point here is the *fact*, that he did so understand that indeterminate phrase, as I shall prove by his own words soon afterwards. Hence it follows, that these *three* sentences and sums in Syncellus, which have been adduced by Petavius and adopted by Marsham, Cumberland and Jackson as convicting the present text of Diodorus of corruption, do in reality rather *confirm* than *impeach* its integrity; since it appears, that they are not strict *quotations* from Diodorus, but only *conclusions* by Syn-

war;" but employs an ambiguous phrase, which *may* mean either way or neither. Jackson's reason then for understanding Ctesias to reckon *from Belus* is very weak; but Syncellus made use of a better one, which was, that if those 1360 years were not reckoned from Belus, then, 1000 years could not be expired at the Trojan war, under the twenty-seventh or twenty-eighth king, as we shall see.

Syncellus himself, deduced from Diodorus, and affirming no more than this, "that Ctesias, as quoted by Diodorus, *could* reckon only 1306 years from the second king Ninus, because he actually *had* reckoned no more than 1360 from the first king Belus. But although Syncellus thus understood Ctesias as computing his 1360 years from Belus, yet it does not follow, that this was the real meaning either of Ctesias himself, or even of Diodorus; and in fact, with respect to the latter, there are two circumstances which incline one to think that he rather conceived them to be reckoned from Ninus only: for he reckons up here the same number of generations, *viz.* 30, as he does afterwards expressly from Ninus; and moreover, he never once mentions the name of Belus, but seems to consider Ninus as the first king; and still farther, if the *second* sum in Diodorus be genuine, which assigns 1400 years from Ninus, he would be the more ready to conceive, that the 1360 of Ctesias could not begin at an earlier king than Ninus. But Syncellus, however, may have neglected to notice any of these particulars, his attention being plainly fixed *solely* on that *first* sum and sentence from Ctesias, which he has quoted word for word, and appa-



apparently collected the *opinion* of Diodorus altogether from *that*. With respect to Ctesias, therefore, the misconception of his meaning, either by Diodorus or Syncellus, is no proof from what king Ctesias himself meant to reckon those 1360 years; and Diodorus may have been just as negligent in observing this, as he has certainly been deficient in not reporting it in a clearer manner than by that ambiguous and indeterminate phrase, *the dominion of the Assyrians*. I have before pointed out another example, where Diodorus, in the hurry of making collections from different historians, has misrepresented the meaning of Herodotus, concerning the word *αὐτονομος*; and instead of its original and primary sense of *independent*, has confined it to that secondary sense of *democratical*, which it had acquired among the Greek states in the later age of Diodorus himself, under the Roman republic. It is at least certain, that all other antient authors place Belus at the head of the Assyrian kingdom, and so might Ctesias have done before them, and nevertheless he might not mean that those 1360 years in question began *so early* as with Belus. Berosus began with Belus and Herodotus also, Cephalion,

lion\*, Eusebius, Augustine, Africanus, and apparently Castor likewise †, and Syncellus himself ‡; and in this they may possibly

\* Κεφαλίων—απο Βηλου και την Τροιας αλωσιν, &c.  
—Syncell. p. 168.

† “ Eusebius—Diodori et Cephalionis dicta productus, in catalogo 37 regum imperium Assyriorum inclusit, annorum vero 1300; Castorem *potius* secutus, cujus etiam testimonium adduxit dicentis 1280 annos regnâsse Assyrios.”—*Sync.* p. 168. Now as Eusebius reckoned those 1305 years inclusive of the 65 of Belus (*vid. proem. apud fin.*), and also those 37 kings inclusive again of Belus, otherwise only 36 from Ninus, how could his 1305 from Belus approach *nearer* to the 1280 of Castor than to the sum of Diodorus, unless Syncellus knew, that Diodorus reckoned *many more* than 1306 from Belus? These words, then, contain a tacit proof that he understood Diodorus to have reckoned 1360 from Belus, though only 1306 from Ninus; and thus they again *confirm*, that Syncellus found 1360 in the text of Diodorus, and that he supposed them to be computed from Belus; otherwise, the 1305 of Eusebius from Belus would be nearly *the same* as 1306 in Diodorus: and that he conceived Eusebius to have reckoned *many less* than Diodorus appears also farther from what he immediately adds after the above words, which I shall notice afterwards.—As for Africanus, see my p. 145, and Syncell. p. 125. On account of the above phrase, *Castorem potius sequutus*, some writers have said, that Eusebius is only a copy of Castor; yet nothing more is here said, than that he approached *nearer* to Castor than to Diodorus in the total sum.

‡ Petavius, indeed, erroneously affirms, that “ Græci ferme Ninum regem primum faciunt.” 9. 13.

But

sibly have all followed Ctesias, although Diodorus, in his brief and perhaps incoherent abstract from *different* authors, has not noticed with what king Ctesias began his Assyrian account, nor yet his sum of 1360 in question. Still farther, beside the manifest *addition* in these sentences of the words *from Ninus*, and the consequent *reduction* of the sum 1360 (supposed by Syncellus to be reckoned from Belus), to the right sum of 1306, if reckoned only *from Ninus*, it may be seen in a variety of other places how common it was for Syncellus sometimes to substitute and sometimes to intermix his own presumptions, interpretations and conclusions (copied indeed chiefly from others) along with the very words, which he found in Diodorus: so that we can never depend upon the sums or words, which he ascribes to that historian in such a confident manner, as to correct his present text by that account of it, which Syncellus presents to us; for he often expresses only the *meaning* in which he

But who are these *Græci*? Only Diodorus, unless we affirm that Justin derived his account of Ninus from some other unknown Greeks, who lived before Trogus. As to Agathias, though he reckons 1306 from Ninus, he does not say, that Ninus was the first king.



he himself understood them, instead of the very *words* themselves of that historian; which, as we have seen, might be capable of different senses, either on account of some ambiguous phrases, or of some apparent incoherence caused by his only partially copying from different authors. We may find another example of this at his p. 151, where Syncellus, in giving us a catalogue of these kings from Berosus, calls the 27th by the name of *Teutamus*, “*Τευταμος ὁ καὶ Ταυτανῆς παρὰ τισὶ λεγόμενος.*” Now did he really find this king to be thus called *Teutamus* in that catalogue of Berosus or Polyhistor, which he here professes to give us? or did he take that name from Diodorus (where we still find it in his present text) and only mean to *explain* who the Teutamus of Diodorus was, by thus identifying him with the Tautanes which he found in Berosus? If the latter, it becomes a clear proof, that the text of Diodorus is correct in reading Teutamus, and nearly indeed the same, if the former case be the real fact; but then how came no such name, but Tautanes only, or Teutæus, to be found in the catalogue of Eusebius? and how came Syncellus himself never any where to place the Trojan war under Teutamus, but only under that Tautanes? Either then he  
must

must have departed from the catalogue of Berosus by arbitrarily inserting in it the name Teutamus, in order to *explain* who the Teutamus of Diodorus was; or else he must have misrepresented Diodorus by ascribing to him as if he had placed the Trojan war under Tautanes, although he does really place it under Teutamus: the former is the most probable, because we shall find reason to think, that the Teutamus of Diodorus actually was not the same king as Tautanes the 27th, but rather as his successor, Teutæus, the 28th king. But either way, it is plain again that we cannot depend on Syncellus as having here scrupulously quoted the *words* either of Berosus or Diodorus, while he has thus left us under this uncertainty; through his intermixing with his *quotation* a doubtful *interpretation* of Berosus or Diodorus from himself or other writers: and afterwards he perplexes again with another of his *interpretations*, by positively asserting, that the 32d king Babius is called the *second Tautanes* [δεύτερος Ταυτανης], which seems again to be only another conjectural *explication* by himself and others; not that he ever found such a name actually ascribed to Babius in the *words* of Ctesias, Berosus, or any other original catalogue. Thus difficult it is to distinguish his real *quotations*

out of those ancient authors from his own *explications*, which he so lavishly intermixes with them! Upon the whole, however, hence arises a strong presumption, that Syncellus had really found the name Teutamus in his own copy of Diodorus, as it is at present; notwithstanding that he every where speaks of the Trojan war as happening under Tautanes, and seems even to attribute this latter name to Diodorus in his quotations from that historian: he may, therefore, in reality, have equally as well found the sum 1360 in Diodorus, notwithstanding that he every where attributes to Diodorus, as if he had, in positive *words*, computed 1300 *from Ninus*; and this only because Syncellus himself conceived those 1360 to be reckoned by Diodorus and Ctesias *from Belus*, which, in course, left only 1300 from Ninus. As then Syncellus might be, and probably was, mistaken in the former case with respect to the *name*, by his conceiving the Teutamus of Diodorus to mean Tautanes, the 27th of Berosus, rather than Teutæus, the 28th; so also he may have been, and not improbably was mistaken also in the latter case, with respect to the *sum*, when he conceived Ctesias to reckon those 1360 years from Belus: for  
they



they might have been computed by that author neither from Belus, nor yet Ninus, but from a later event than either, namely, from the complete *conquest of Asia* by Ninus; to which event the phrase *dominion of the Assyrians*, is equally, or rather more, applicable than to either of the two former ones, and which was exactly 1360 years before the end of the kingdom, according to the computation of Berosus, and likewise Syncellus himself. But whether or not Syncellus was mistaken in both cases, or in neither, yet with respect to the *fact* itself that Syncellus did actually conceive those 1360 years of Ctesias to be reckoned from Belus will clearly enough appear (as I have already mentioned) from what he immediately subjoins after those three pretended *quotations* from Diodorus, at p. 166 and 168: for, in that former page, he immediately proceeds to prove concerning the very *next* sentence, which follows them in Diodorus, and which mentions “there being somewhat *more than 1000 years* from the *dominion of the Assyrians* to the capture of Troy,” that these 1000 years must have been computed *from Belus*; as we have seen in a foregoing note, that Jackson computed them likewise, and ascribed *both* the above

computations in Diodorus to Ctesias. In fact, it may seem plausible to understand those two computations in the two contiguous sentences in the same way, that *both* were reckoned from the same epoch, and taken by Diodorus from the same author: so that if Syncellus conceived the *second* of those two sums to be reckoned from Belus, without any express authority from Diodorus, and only as a *conclusion* of his own from the amount of the *sum* itself being 1000; there can be no good reason, why he should not have equally formed the same *conclusion* concerning the *first* of those two sums; as in fact he evidently did, by his computing no more than 1300 *from Ninus*, for the *addition* of which words he had no authority from Diodorus any more than in the other case. Let us, then, observe how he proves that those 1000 years before Troy were reckoned by Diodorus *from Belus*, immediately after twice *affirming*, that Diodorus computed only 1300 years *from Ninus*; his words are these: “ Diodorus has only made known to us, that the duration of the 35 kings from Ninus \* was more than 1300

\* Here he attributes to Diodorus as reckoning 35 kings *from Ninus*; so he does again in p. 168:  
yet

years; and he says also, that the capture of Troy was under the 26th king over the Assyrians from Ninus [called Tautanes or Teutamus], after 1000 years and more were expired, thus inserting in his own account what he had found said by others, yet not, however, as having every where made them cohere well together: for, in fact [as one example of discordancy], in the present case of the capture of Troy, the *time* of Tautanes coincides with the 942d year of *the dominion of the Assyrians* [ασσυρίων αρχῆς] Tautanes being reckoned the 27th from Belus [and that those 1000 years must have been reckoned from Be-

yet we cannot conclude from this, that the present text of Diodorus, which has only 30, is corrupt; and the same afterwards, that 20 *from Ninus* in the present text is a corruption instead of 26th *from Ninus*. For it is evident here again, that Syncellus does not confine himself to a *strict quotation*, he having said 35 *from Ninus*, although Diodorus says 30 *from the dominion of the Assyrians*: so that here again Syncellus takes upon himself to *explain* this latter phrase to *mean* from Ninus; in this *explication*, then, Syncellus may just as well have altered the *sum* 30 as the *epoch* from which they are reckoned is certainly altered, and thus have only *presumed*, that Diodorus *meant* 35 from Ninus, although he found in the text 30 *from the dominion of the Assyrians*, as at present. Such presumptions and interpretations by Syncellus himself are to be found in every page.

lus



lus is evident], for if we were to cut off the 55 of Belus from the 942 years above mentioned, this sum would, according to *accurate* computation, be reduced to very many years less indeed than that larger sum of 1000, or rather than the *more* than 1000 years in Diodorus, and also in fact than the true time of the capture of Troy. Hence, then, it appears, that I myself, and those authors whom I have followed in this, have [in order to make up fully the more than 1000 years of Diodorus], rightly fixed the true capture of Troy at *nearly about* the 1100th year of the *dominion of the Assyrians* [ασσυριων αρχης] under the 32d king, Babijs, and in my own year of the world 4329." \* Now in the above sentence

\* Ταυτα ο Διοδωρος ημιν παραδιδωσιν, μηδ' ουτω θαρρησας αποφηνασθαι τον των βασιλεων αριθμον, η χρονου την ΑΚΡΙΒΗ ποσοτητα, προσηγοριας τε αυτων, και ΕΠΙ ΤΙΝΟΣ η κατα Τροιας των Αχαιων εφοδος γεγονεν. αλλα παντα φειδομενος δια το των παλαιωτερων αυτου ιστορικων ασυμφωνον, μονον δε οτι πλειω των ατ ετων ο χρονος των απο Νινου λε βασιλεων εστιν, απεφηνατο. Και οτι την Τροιας αλωσιν επι του κς απο Νινου βασιλευσαντος Ασσυριων [ταυτα μοι] φησι γενεσθαι μετα α ετη πλειω, αλλοις επιγραφων τα λεγομενα, ως μη παντη συντιθεμενος. ο γαρ Ταυτανου χρονος τω θμβ ετει συντρεχει της Ασσυριων αρχης, κ και ζ λογιζομενου Ταυτανου απο βηλου. Ει γαρ του Βηλου γε ετη αφελωμεν εκ των θμβ, πολλοις ετεσι κατα τον ΑΚΡΙΒΗ λογον ελαττωθησεται εκ των α πλειω χρονων η μετα α πλειω

tence we find it first of all clearly proved, that Syncellus means here by *dominion of the Assyrians*, the accession of Belus, although in other places, and particularly in note to p. 187, he means by that phrase the accession of Ninus. Diodorus likewise employs the similar phrase [ασσυριων ἡγεμονια] in the same variable manner; for, in his *second* sentence (vide my p. 164), it is united with the words *from Ninus*; in two other places, however, no king is mentioned: yet in regard to that *third* passage, quoted at my p. 168, Syncellus has here proved, that Diodorus must there have meant *from Belus*; therefore in his *first* passage taken from Ctesias may be meant neither of those kings, but the true and *real dominion* of the Assyrians over Asia, when its *conquest* was completed. So that the phrase in question of itself denotes no one particular *epoch* of commencement, but *may mean* any such as the sum or words united to it shall point out and

πλειον κατα Διοδωρον, και την αληθη αναλωσιν Τροιας. καλως αρα ἡμιν εστοιχειωται και τοις εκθεμενοις, εγγυς που τε αρ ετους της Ασσυριων αρχης κατα το δεκα ετος κοσμου η Τροιας αλωσις, επι του λβ βασιλεως Ασσυριων Βαβιου, ος κατηκολουθησαμεν.—P. 166. N. B. The translator supposes [ταυτα μοι] to be a redundancy; it is more probably a corruption either of Ταυτανου or Τευταμου.

and require; and that, in the present passage from Syncellus, they mean *from Belus*, will appear still more plainly, if we examine the contents of it more minutely. Now, instead of 942 [9μβ], as in the present text, the Latin translator proposes by conjecture to read 909 [909]: this is, in truth, the right sum up to the *beginning* of the reign of Tautanes; but why might not Syncellus as well by *time* mean the *end* of this reign, which would be 940? and, indeed, his reasoning rather demands that he should do so; for even thus there would still want 60 years of the 1000 of Diodorus, and his reasoning would thus be more accurate: so that there is no good pretence for altering the sum in the text here, as it exceeds the true sum only by 2; nay, even this may not be a manuscript error of the text, for in the 21st reign, Eusebius has 2 years more than the catalogue of Syncellus, which would make up the full sum 942, and there were apparently other variations in the sums of reigns in different catalogues. Syncellus, therefore, being sensible of this deficiency of 60 years in the sum 942, proposes, agreeably to the opinion of some preceding chronologers, to delay the time of the capture of Troy until the later reign of  
Babius,



Babius, the 32d king; which, he says, would be *nearly about* the 1100th year of the *dominion* of the Assyrians. But this indeterminate phrase still leaves a doubt, whether here again he reckoned these 1100 years, as before, from Belus, or whether from Ninus; this doubt, however, may be removed, and it may be proved, that here again he reckoned from Belus, by means of the following circumstances: for he calls Babius the 32d king, but this is true only on the supposition of Belus being the 1st, otherwise from Ninus he was but the 31st. There is also another and more decisive proof of his computing from Belus, which may be deduced from his own year of the world 4329, in which he places the capture of Troy: for it appears, at his p. 155, that the year 4329 was the 5th of king Babius, and this was *nearly about* 1100 years from Belus [*εγγυς του*], namely, 1114 years; but there would be only 1059 years to the 5th of Babius, if reckoned from Ninus, which would want almost as many years of 1100 as 942 did before of the 1000 years of Diodorus. It is plain, then, that throughout the present passage of Syncellus, he every where by *commencement or dominion of the Assyrians*

means the accession of Belus, notwithstanding that, in the quotation from him, at my p. 187, he meant by the same phrase the accession of Ninus. Now, since Syncellus thus understood Diodorus, in his *third* sentence, concerning the 1000 years before Troy, to *mean* that those 1000 years of the *dominion of the Assyrians* were reckoned from Belus, why should he not equally have understood Diodorus, in his immediately preceding *first* sentence concerning the 1360 years which Ctesias computed from the *dominion of the Assyrians*, to mean in like manner, that they were reckoned from the accession of Belus? which if he did, then he could not *conclude* otherwise than he has done, "that Diodorus computed *more than* 1300, *i. e.* 1305 from Ninus," this being the remaining sum, after deducting the 55 of Belus. So that this last computation is not a strict *quotation* of the very words of Diodorus, but rather a *conclusion* by Syncellus himself, rightly deduced from *his own* conception of the information by Diodorus, that Ctesias assigned 1360 years to the *Assyrian dominion*; meaning *from Belus*, as Syncellus understood Diodorus, although Ctesias himself *might* really mean

mean only from the *complete conquest* of Asia by Ninus.\* If then any readers should think that I have dwelt too minutely upon this subject of the real epochs of the computations which Syncellus ascribes to Diodorus; yet they should recollect, that it is what the untrue assertions and inconclusive reasonings of several very learned moderns have necessarily required of me; that I might not seem either superciliously  
to

\* It may be observed, therefore, *in general*, that when Syncellus employs the phrase *dominion of the Assyrians* in the passage quoted at my p. 187, it is there so limited and tied down by the addition of *from Ninus*, as to be incapable of any other epoch of computation: but when it is not thus tied down to *some one* reign, Syncellus apparently in such cases *always* means by that phrase the accession of Belus, which likewise in all other authors is accordingly considered as the *commencement* of the Assyrian dominion. The case is much the same with Diodorus; for, in his *second* computation of 1400 years, his phrase *dominion of Assyrians* is expressly limited by the words *from Ninus*; but in the other two places, where it is not limited by mention of the *name* of any king, there, like Syncellus, he *might* mean *from Belus*. So that, strictly speaking, we cannot, perhaps, say that either author employs *dominion of Assyrians* in any variable manner, but rather always as meaning *from Belus*, unless there be some words *added* to limit and determine the phrase to mean some other king or epoch of computation.



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to have passed over without notice, or to have insufficiently examined the force of what they have urged against the integrity of the present text of Diodorus: but which I have thus proved to consist of nothing except rash conjectures, or manifest mistakes, or downright misrepresentations, or else want of due attention to the *whole* of the evidence to which they have appealed, as contained in the work of Syncellus; and which does, in truth, require still some farther remarks, in order to present to readers a clear view of what information is to be collected from this author, and in what it is deficient.

I must, therefore, notice farther, that, although in the above passage Syncellus has rightly proved, that the *more* than 1000 years before Troy mentioned by Diodorus, would not be expired at the *end* of the 27th reign of Tautanes, even although they commenced *so early* as the accession of Belus; yet it does not follow hence that he ought to have deferred the capture of Troy to *so late* a reign as the 32d under Babius at the expiration of above 1100 years; it would have been sufficient, if he had deferred it to only a single reign *later*, that is, to the end of the 28th reign of Teutæus: for this name has even more re-  
sem-



semblance to *Teutamus* than *Tautanes* has, and the identity of these two kings would have spared the necessity of that arbitrary invention of *Babius* being called by some persons the second *Tautanes*; which has all the appearance of a rash and forced conjecture, countenanced by no other antient authors, and apparently affirmed for no other reason, than that *Babius* might under this second appellation of *Tautanes* have some similarity to the name of *Teutamus*. The time also of the 28th reign would have been apparently suitable enough to the interval of 1000 years mentioned as having elapsed before *Troy*; not, indeed, altogether with the later and more exact catalogue of the reigns by *Berosus*; but if that sum of 1000 was taken by *Diodorus* from *Ctesias*, *Hellanicus*; *Herodotus*, or more antient historians of the Assyrian kingdom, it might, in such case, have agreed perfectly well with the 28th reign of *Teutæus*, which ended with the 984th year from *Belus*, in the catalogue of *Berosus*, who allowed only 55 years to *Belus*: more antient authors, however, seem to have allowed a longer reign to him; for we find that *Eusebius* assigned to him 65 years, which he might have copied from *Ctesias*, *Hellanicus*, or some more antient writer

writer than Berosus. There is also some reason to think, that Africanus had allowed him 62 at least, for we find this sum in an old barbarous Latin Chronology, published by Scaliger, in his edition of Eusebius, which was almost a translation from Africanus, as I might easily prove. Now, by the addition of those 10 years, the interval would amount to 994: but, then, Diodorus does not say, that Troy was *taken* in the reign of Teutamius; he only says, that in this reign “the Greeks under Agamemnon *made war* against Troy” [Ἕλληνας ἐπὶ Τροίᾳ στρατευσάαι], and as the siege lasted 10 years, these would augment the 994 to 1004. So that if the war began under Teutæus, and only 3 or 4 years before his end, it would not be finished until the 6th or 7th year of his successor in the 1003d or 1004th year of the kingdom, agreeably to the account of Diodorus: for it is possible, that his date of 1000 may refer to the *end* rather than to the *beginning* of the war, notwithstanding that his words, through brevity, do not with certainty determine this; but at least Syncellus all along understands the 1000 to mean the *end* or capture of Troy (ἄλωσις), and it is to this his reasoning relates. Agreeably to this, the above old Latin translator of Africanus fixes the event,

event, that is, the beginning of the war, *late* in the reign of the king, supposed to be Teutamus, *i. e.* in his 32d year, which would be only a few years too soon. Not, however, that this is to be considered as being the actual historic truth, nor yet even as a plausible tradition, of which apparently none remained either in Greece or Asia: but only as being merely the computation, date, reign and manner in which the *earlier* Greek historians Ctesias, Hellanicus or others *before* Berosus had fixed the capture of Troy in those first Greek accounts of the Assyrian kingdom, while *many* of the Greeks still computed the interval between Troy and the Olympiads at so many as 520 years; which accounts Diodorus may have here followed in preference to the *later* and more exact one of Berosus. Neither Syncellus, therefore, nor any of the moderns have in reality convicted Diodorus or his present text, of any error or incoherence in regard to this sum of 1000 years before Troy, any more than with respect to the 1360 years of Ctesias for the Assyrian *dominion*; for thus *both* those sums are consistent with each other, and also with the 1400 years which Diodorus afterwards allows from Ninus to the end of the kingdom, as well as with the

the



the total sum 1460: and the real fact seems rather to be, that Syncellus, and those authors whom he professes to follow, did not any more than modern authors, rightly comprehend, either *from* what event those 1360 years *began* to be reckoned, nor yet *with* what king those 1000 years *ended*; which might thus be neither Tautanes, the 27th, nor Babius, the 32d, but Teutæus, called Teutamius by Diodorus, the 28th reign. There is, moreover, another circumstance, which confirms this 28th king to be the real Teutamius of Diodorus, beside the similarity of the *name* and the agreement in point of *interval*; which is, that it is *immediately after* Teutæus, that the four reigns are omitted in Eusebius and in the old Latin translation of Africanus; and which had been apparently thus omitted before by the Greeks, in order to reduce the interval between Teutæus, *i. e.* the capture of Troy, and the first Olympiad, to 407 years. This indicates, that those Greeks who made this reduction and omission of four reigns, conceived Troy to be besieged under Teutæus, and therefore did not shorten the interval until *after* the end of his reign; so that hence again it follows, that the Teutamius mentioned by Diodorus, under whom *began* the war

war of Troy, was even considered by the Greek chronologers in the age of Alexander as being the same king with Teutæus, not with Tautanes his predecessor. Consequently the pretended incoherence, which either various learned moderns or Syncellus before them, have imputed to the account and sums in Diodorus, arose only from their own mistakes, and not from MS. errors in the text of Diodorus, as it stands at present: this will also appear still farther to be the real fact, and it will likewise throw farther light upon the whole subject, if we attend to the *observations*, which Syncellus himself makes upon the Assyrian account as given by Diodorus; and which he has immediately *prefixed* as a kind of preface to the passage last quoted from him at my p. 200.\*

My

\* If I were to take notice of all the mistakes of different modern authors, I should never finish, but it may be acceptable, if I here subjoin one other example, as it materially affects the subject treated of above, and is included among several others in a long note by Jackson, at his p. 245, vol. i., where he says, "That in the following words quoted by Syncellus from Cephælion, *Μητράϊον τὴν ἀρχὴν διαδέχεται Ταυτάνος*; Z. (p. 168) he might easily have seen, that Cephælion wrote KZ; i. e. 27, and thus made Tautanes the 27th from Belus, as he really was; from Belus therefore it was, that Cephælion supposes the 1000 years before Troy to be reckoned." Jack-

Vol. III.

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son

My object hitherto has been to prove,  
that there is in fact no such *necessary in-*  
*coherence*

son then here again conceives, that Cephalion as well as Syncellus and himself computed those 1000 years from Belus, just as I have proved above, that Syncellus conceived Diodorus to do likewise. But what I intended principally to observe was, the example which we have here of the very precarious nature of all conjectural corrections of the text of ancient authors: for as the above solitary Z can mean nothing by itself, and as Tautanes was certainly the 27th king, it was rather a plausible conjecture here of Jackson, and, indeed, of Scaliger before him, that a K was omitted out of the text, and much more plausible than that of the editor Goar, that we should read KB, *i. e.* 22. But behold the fate even of plausible conjectures! for there is only one complete MS. of Syncellus extant, *viz.* in the Royal Library at Paris, from which Goar's printed edition was made; some portions of it only are extant in the libraries of Italy. About 40 years however after that edition another MS. was sent to Paris from Constantinople, said to have been found in the seraglio there by the French ambassador; but it is defective both at beginning and end, yet the middle, which contains the most material portion of prophane chronology, is well preserved: proposals have been twice made at Paris to publish the variations of this MS. from the printed edition, but never yet executed. In one of those proposals published in the *Journal des Sçavans*, some specimens are given of a few of those variations, and among them is one concerning the very sentence, where the above solitary Z occurs, and where instead of it the word ζω is found; which makes good sense with what follows, and is, indeed, necessary to the sentence,

*viz.*



*coherence* between the three sums in the Assyrian account of Diodorus, as all the moderns, except Vignoles, have supposed; therefore that no argument can be brought in favor of any conjectural alteration of those sums in his present text, on account of such a pretended incoherence as *may not* in reality exist between them. However I allow that Syncellus did unite with the moderns in the same opinion, of the Assyrian account in Diodorus being very *incoherent*: but then I contend still farther, that this fact, instead of proving that the *present text* of that historian is corrupt, does, if rightly considered, prove the directly contrary; as we shall easily perceive if we attend properly to the *observations* above-

F f 2

mentioned

viz. *Ζαν και αυτος κατα τα εδη Ασσυριων*: so that the supplement to Z is to be made after it, and not by K before it. Who could have suspected such an accident, as that Tautanes should thus happen to be the 27th king, and thereby afford a very plausible foundation to mislead from the right path all the great conjurors at conjecture, Scaliger, Petavius, Jackson and others, who have all adopted that supposed correction of the text? only with this difference indeed, that Jackson supposes it to be an error made by Syncellus himself in his extract from Cephalion, while his predecessors suppose it to be a MS. error in the present text of Syncellus, as in fact it is thus proved to be, yet different still however from what they conjectured.

mentioned of Syncellus, concerning the Assyrian account of Diodorus, in which he states the nature of that incoherence or inaccuracy, to which he objects. He says, "Such as above-mentioned is the account, which Diodorus has given us, he having thus neither ventured to make known the *exact* number of Assyrian kings, nor the *exact* lengths of their reigns, nor yet their names, nor under which Assyrian king *exactly* it was, that the expedition against Troy took place; but having delivered a scanty and imperfect account of all things, by reason of the discordancy of the historians before himself, he has only informed us, that the total duration of the 35 reigns from Ninus amounted to more than 1300 years: and he says also, that the capture of Troy happened under the 26th from Ninus [Tautanes or Teutamus] after more than 1000 years were expired; thus inserting in his own account what he had found said by others, yet not as attempting to make all things *cohere* well together," &c. Now if this translation expresses the true sense of the Greek words, and if we compare what Syncellus says here concerning the *imperfect* account of Diodorus with his present text, and with what Syncellus calls a more *exact* account, meaning doubtless that

that which he himself has adopted from Berosus and Polyhistor; we may so far allow that there is some *appearance* at least of truth in these *observations* concerning Diodorus, and that they are agreeable to the opinion of the moderns: but is not this very circumstance a good reason why we ought to preserve the present text of that author inviolate, although it should seem *incoherent* as well as *imperfect*, rather than as Jackson and others before him have proposed, to alter by conjecture the *sums* there, in order to force them into an agreement with the more *exact* account by Berosus, with which Syncellus here affirms, that they did not agree in his own time and copy? This would be to set the present text of Diodorus at direct variance with the above information of Syncellus concerning it; and this under pretence of making it more *exact* and *coherent*, notwithstanding that we are here informed, that it was not found so in the copy of Syncellus, either with respect to the accounts of others, or even itself, “Diodorus not having actually and originally made all things cohere well together.”

ὡς μὴ παντὶ συντιθεμενος. That this is the import of that phrase appears from the example which he immediately subjoins to prove the truth of his assertion, namely, “that  
Diodorus



Diodorus has placed the capture of Troy under the 27th king from Belus, the 26th from Ninus; and yet affirmed in *contradiction* to this, that it happened at more than 1000 years after Belus," although in reality even the *end* of that 27th reign was 60 years short of 1000. This contradiction however, arises solely, as I have proved, from the erroneous supposition of Syncellus himself, and such other authors as conceive the Teutamius of Diodorus to be the 27th king Tautanes, instead of the 28th Teutæus: so that it is an inconsistency created altogether by the expositors of Diodorus, and not subsisting in that historian himself; nevertheless it equally well ascertains to us what sense Syncellus affixed to his word *συγχεσμενος*, and thereby proves, that if in his time the text of Diodorus was not considered as being either *exact* or *coherent*, it was the more similar to our present text of that historian; which consequently demands no correction in these respects, as this would be to render it *different* from what it was formerly.\* So that this very circumstance of

\* For the Greek text of this passage see my p. 200, concerning which we may first observe, that the Latin translator understands *συγχεσμενος* as meaning, that Diodorus did not himself *assent* to all the accounts,

of some *appearance* at first sight of incoherence and inaccuracy in the Assyrian sums of

accounts, which he copied from others : this makes no material difference with respect to the conclusion of incoherence deducible from the whole ; for there was still an *incoherence* subsisting, namely between Diodorus and preceding authors, consequently between the several accounts themselves relative to the date of the Trojan war and other events. But there are some other sentences in my translation, wherein the sense differs more materially from that of the Latin translation, and which it is necessary to point out in order to ascertain the true meaning of Syncellus ; for the Latin translator has not given a coherent sense to the whole passage, which indicates that he did not rightly comprehend the import of some parts of it. For example, to whom do the words *ἐπὶ τῷ* refer ? The Latin translator supposes to the commander of the Greeks against Troy, for he renders that sentence “ sub quo imperatore Græcorum.” But it is not true, that Diodorus has omitted to mention the name of the Greek commander Agamemnon ; and Syncellus himself had but just before expressly quoted from Diodorus, “ that the Greeks made war against Troy under Agamemnon.” It may in like manner be said, that Syncellus could not mean to refer to the Assyrian king, in whose reign Troy was taken ; for he had just before quoted also as from Diodorus, “ that this event happened under Tautanes the 26th from Ninus.” Therefore the only consistent and apparently his real meaning seems to depend upon the word *ἀρξίστην*, which the Latin translator has wholly omitted, and which might be intended to extend its force to the *preceding* part of the sentence as well as the *succeeding* ones, and thus to affirm only, “ that

Diodorus

of Diodorus, either with respect to one another, or the more *exact* account of  
Berosus

Diodorus had not given an *accurate* account of the articles in question, neither of the *total duration* nor *number* of Assyrian kings, nor of the *date* of the Trojan war, nor under what Assyrian king *exactly* it was [ἐπὶ τίνος] that Troy was taken." For Diodorus calls him *Teutamus the 20th king*, without determining whether or not this Teutamus was the same person as Tautanes the 27th of Berosus, or as Teutæus the 28th, or any later king: so that these words of Syncellus here, "Diodorus has not stated under what king *exactly* Troy was taken," notwithstanding he had just before said expressly as from Diodorus, *that Troy was taken under Tautanes the 26th from Ninus*; these words, I say, may possibly confirm what I have advanced already from another passage of Syncellus, that he actually found *Teutamus* in the text of Diodorus, just as in our present text: and that the *Tautanes* mentioned by him elsewhere was only the *interpretation* and *explication* commonly given by other writers, as to what king Diodorus *meant* by Teutamus, among whom not improbably he might refer to Africanus and Eusebius. Or else he might mean to censure Diodorus for placing the event under his Teutamus and not under Babius the 32d, whom he calls the 2d Tautanes; but whichever sense he meant, the word *exact* is either way necessary, it being not for any *omission* that he could consistently with truth in this article censure Diodorus, but only for a want of *accuracy* in what he did say relative to the fact in question. The cause of this real or supposed *inaccuracy*, Syncellus has also shewn, namely by Diodorus having followed the accounts of different authors, who did not agree together; for the information



Berosus (which the moderns have made their chief pretence for correcting his text) is thus proved by Syncellus himself to afford no better foundation for correction than all their other objections; but to be on the contrary rather an evidence in favor of its integrity, by its being thus shewn to have still the very same incoherent and inaccurate *appearance*, which Syncellus had found it to have in his own copy of that historian 1000 years ago: nevertheless, such ill-founded accusations and ill-judged corrections, are now in general received with so much favor by the public, that all dabblers in antiquity take upon themselves to alter the antient text of authors at their pleasure, when, at the same time, they are only displaying their own imperfect knowledge of the subject.\*

Thus

tion of the earliest Greek historians, Hellanics, Ctesias, or others, apparently differed from the more *accurate* account given afterwards by Berosus: which latter Syncellus himself must have most approved, as he has adopted it, and upon the foundation of its computations being right, has objected to those of Diodorus and others. The former differed in allowing 65 to Belus instead of the 55 of Berosus, which is confirmed by Augustine, who allows also 65 to him; this, if copied merely from Eusebius, yet proves that it is not a MS. error in Eusebius. *Civ. Dei.* 20. 21.

\* It may for the same reason be proper to state  
VOL. III. G g here

Thus then, after having analyzed this pretended *quotation* by Syncellus out of Diodorus,

here the nature of some unjust accusations which have been made against Eusebius, that readers may be the better able to distinguish between such as are true and such as are not. While the Assyrian sums in Diodorus have been accused of having been made *longer* by copiests than in the original text, the directly opposite accusation has been made against Eusebius, "that he had intentionally and unfaithfully made the Assyrian period *shorter* than he had found it in his prophane authorities, in order to suit the better with his own scripture chronology." Jackson, p. 245. Now this accusation had indeed been made before in a certain degree by Syncellus, and adopted by Scaliger; but then this related only to his *omission* of the four Assyrian reigns after the Trojan war, containing 162 years: and by this Syncellus clearly did not mean to accuse Eusebius of having been the *first* to introduce that omission; for at the same time he shews, that Castor and Cephalion had before Eusebius *shortened* the Assyrian period as much, which must have been done by a similar omission of those four reigns. Syncellus therefore could only mean to blame the judgment of Eusebius, in preferring to adopt such a *shorter* period to the longer ones, and this in order to be more *conformable* to his own scripture chronology, on account of *his having made a similar omission there by not including Cainan* [δια το συμβαλλεσθαι τω παραλογισμῳ, μη χρονολογησας Καϊναν] p. 168. As it is still a question not decided even among the moderns, whether that Cainan ought to be included or not, this accusation therefore only affects the judgment not the fidelity of Eusebius, for *omitting* those four reigns as several prophane authors

Diodorus, and compared together the *whole* of what he says on the subject, I have proved,

authors had set him the example of doing ; and he certainly had a right to choose out of several authorized periods that, which in his opinion suited best with scripture, as hereby he *shortened both* systems of chronology alike. Thus far however the accusation in the above quotation from Jackson may be true ; but Jackson carries it farther than Syncellus and Scaliger, namely, that beside the above *omission* " Eusebius has *shortened* several other reigns to the amount on the whole of 62 years." *P.* 250. *note 23 and 29* : he therefore gives us there another catalogue of Assyrian reigns compiled by himself out of all the ancient ones, in which he boasts " of having supplied the defect of those 62 years in Eusebius, who reckoned no more than 1240 years from Ninus, though he knew and owned that Ctesias computed this period to be 1300." This is quite a different accusation from the former one of Syncellus and Scaliger, but one which has not the least truth in it, and against which it may therefore be proper to caution readers ; for as Eusebius allowed 65 to Belus (*v. proem.*) these would augment the 1240 to 1305, and on adding 162 for the four omitted reigns, they will amount to even 1467. But all this concerning the *total sum*, Jackson omits to mention, and only observes, that in *several particular reigns* the catalogue of Eusebius has a few years *less* than Berossus in Syncellus, all which *deficiencies* added together make up 62 : and he takes no notice, that these *deficiencies* are in Eusebius himself already compensated by as many *excesses* above the sums of Berossus in *other reigns*. Thus the 12th and 13th have 30 and 20 in Eusebius, while in Berossus they have 28 and 22. So the 15th



proved, that it is not in reality a strict quotation of the very *words* of Diodorus; but only an explication of the *meaning*, which Syncellus himself affixed to them, and a *conclusion* of his own, deduced from that sense, which he understood Diodorus to have given to the indeterminate phrase *dominion of the Assyrians* in the three places, where Diodorus employs it.\* So that instead of depending so much, as many very acute moderns have done, on this pretended quotation in order to impeach the text of Diodorus; nothing more was necessary to discover their error than to state,

as

and 16th have 40 and 40 in Eusebius, but in Berosus 42 and 38. The same in several others; thus the 27th has only 40 in Eusebius where Berosus has 44; but this is compensated at last, for Sardanapalus has 20 in Eusebius, but only 16 in Berosus. Thus are new books filled with new errors, which require a multiplicity of words in others to point them out, and the effect is, that readers must be either misled or tired out. On a balance of all the sums Eusebius has one *more* after the 28th king, and five *less* before him; but then he gives 10 *too many* to Belus; so that on the *whole* the sums in Eusebius are *too long* rather than *too short*.

\* The very same is probably the origin of the 1306 years in Agathias ascribed by him to Ctesias, namely that he understood Ctesias to reckon 1360 from Belus, therefore as he concluded only 1306 from Ninus: so that the text of Agathias is no more corrupt than that of Diodorus.

as I have done, the true nature of the words and reasoning of this obscure author, to whom they have appealed : whose reasoning is indeed often of such a muddy, ill-connected and disjointed complexion, that it is neither easy in all cases to comprehend what he means to prove, nor yet even easy to understand what he means to affirm ; beside the many deficiencies and corruptions in his text, so that what he affirms in one passage, his present text seems to contradict in another. But upon a view of the whole, when compared and connected together, he is found rather to *confirm* the present text of Diodorus than to impeach its integrity. I have shewn also, that this text of Diodorus *may be* understood in such senses, that the different computations contained in it are not *necessarily* inconsistent, but rather only *different portions* of the same whole sum of 1460 ; and that his *second* sum of 1400, if the text be genuine here, by its being reckoned only from Ninus, certainly proves even the sum of 1460 from Belus to have been *known* by the Greeks before the age of Diodorus : but that if the reading in this place be a corrupt one instead of 1300, yet still his *first* sum of 1360 proves at least a longer sum than 1300 to have been *known* as the duration of

of the Assyrian kingdom before the age of that historian ; there being no variation in the MSS. with respect to the sum 1360. It follows therefore hence, that not any one of the *several objections* made by Petavius or others against the present text of Diodorus have any good foundation ; and that this *last* deduced from a pretended *quotation* out of Diodorus by Syncellus is just as insufficient as all the other preceding ones. Here however it ought to be remembered, that I do not myself maintain, that the 1360 of Ctesias are to be understood as being computed from the *conquest of Asia* by Ninus ; but that I only shew this to be a possible and not improbable epoch for that computation : I therefore propose this, after Vignoles, to those who think it necessary to find the *three* sums in Diodorus consistent with one another and with those of Berosus ; as being a more rational mode of reconcilment, by thus only varying the application of the ambiguous phrase *dominion of the Assyrians*, than by forcing an agreement by altering the sums of the present text by conjecture and in an arbitrary manner against the uniform consent of all MSS. As to my own opinion I think it a very sufficient solution of any *apparent* discordance in the three sums, to have been informed



informed by Diodorus, that he extracted his account from different authors, who did not agree together; and we have found such an *apparent* discordance to have subsisted as early as the age of Syncellus, therefore probably from the very first, whether it be a real discordance or not.

But here it is observable still farther, that all the moderns, except Jackson, who have so implicitly adopted the *unsolid objections* of Petavius, have nevertheless omitted to notice the only real proof against the present text of Diodorus, which can be produced; which is, that although all the MSS. have uniformly 1360 in the *first* sentence; yet in the *second* of 1400 two MSS. are found to have only 1300.\* Now Weseling has informed us which these two MSS. are; he denominates them *Mutin*; and *Gemist*; and certainly the right text is

\* “ All the present copies of Diodorus read 30 *kings*; only from Ninus—and the term of the empire is stated at *more than* 1360 years in one place and *more than* 1400 in another; but that these sums both of reigns and years are corrupt is certain—for that the words *ετι δε εζηνοντα* are an interpolation is plain from the passage afterwards where they are left out, and where two MSS. have only 1300, as in the first passage, although most copies have 1400; the former is undoubtedly the true number. Syncellus has *cited* those words rightly both as to the number of reigns and years.” *Jacks. v. 1. 244.*

is not to be determined always by the majority of MSS., it being often found that a single one has preserved better readings than the majority; but then the age, character and credit of such dissentient MSS. ought to be well examined, before they be preferred to the majority; of which there is no account in Wesseling, unless I have overlooked it.\* However, the mere doubt itself thus created by the variation in these two MSS. operates thus far at least, that we ought not to produce the *second* sum

1400

\* Unless Wesseling has given information somewhere (which I have not found) what his reference by *Gemist*, means, it may be a doubt, whether it be in reality a reference to any MSS., or only to a passage in some of the Greek works of Gemistus Pletho, a refugee Greek at Florence so late as the year 1500: one is *De philosophiâ Platonis*, another *De gestis Græcorum post pugnam ad Mantineam*; another an *epitome rerum sicularum*. But even if any real MS. be referred to by the name *Gemist*, yet it can receive no additional authority from its having merely passed through the hands of so late an author as Gemistus, unless it be an ancient MS. which he brought with him into Italy, written before the art of printing was introduced; yet possibly it may be only a late copy from the other styled *Mutin*, of which I know nothing, and its character ought to be better ascertained: it ought especially to be observed, whether Wesseling elsewhere makes use of these MSS. or prefers them to others, otherwise they have no claim to preference here.

1400 as a *certain* proof of a longer sum than 1300 having been known to the Greeks before Diodorus, until the value of these two MSS be decided: but if this doubt should be even decided in favor of 1300, still it is not inconsistent with the *first* sum 1360, because these 1300 are expressly said by Diodorus to be reckoned only *from Ninus*; whereas the foregoing 1360 are understood, as I have proved, by Syncellus to have been computed *from Belus*. Thus 1300 or 1305 in the *second* sentence would become an additional proof, that 1360 in the *first* sentence were right and reckoned from Belus by being about 55 years more; and at the same time would prove, that Diodorus supposed this as well as Syncellus. So that *either* way these 1360 remain an unmoveable obstacle to the pretence of Cumberland, that the *elder* ancient authors favor the shorter sum of 1300; this being a more ancient testimony by 200 years than any evidence now extant for 1300: thus it gives *additional* support to the still more ancient testimony of Berosus to a longer sum than even 1360; which was what I proposed to prove. Fortunately therefore the works of Diodorus and Syncellus have been preserved, which thus disprove the visions of modern commenta-



tors on the Jewish scriptures, that the 1300 years of the Assyrian kingdom must be the whole sum, and that it agrees with and confirms the computations there concerning the deluge and the age of Nimrod. Let us indeed seek after truth, but not be the inventors of imaginary visions; the most ancient sums must be certainly the original ones, and these we have found to be the longest ones: while of such a mutilated and shorter sum as 1300 no traces are now found until 600 years after Ctesias; although I presume, that it was indeed formed as early as by Eratosthenes or his followers within a century at most after Alexander, as I shall farther shew afterwards, together with the causes, which produced it.

While the *originality* of the longest sums is thus proved together with the *integrity* of the text of Diodorus, the *fidelity* of Eusebius becomes at the same time manifest, since he adopted the Assyrian sums which were most in credit in his own *later* age; although indeed his *judgment* may be still censureable, in preferring the mutilated sum before those more genuine ones, which he might have found in the more ancient authors Berosus and Polyhistor: yet even this his erroneous judgment admits of  
excuse,

excuse, if we consider the great reliance, which has been placed even in modern times upon the integrity of the Hebrew text; and also the prepossession, which has prevailed in our own age as well as in his, that the computations of the Hebrew text concerning the deluge and Nimrod, admitted of confirmation from the prophane chronology of Belus, and that they were indeed the same persons; for the time of Nimrod disagreed irreconcilably with any longer Assyrian period than 1300. Nay, Petavius also is not without excuse thus far, and much more than Marsham, Cumberland and Jackson, as may be seen in the subjoined note :\*

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equally

\* The whole work of Syncellus had not been published, when Petavius wrote his *Doctrina Temporum* in 1627; so that he could only make use of the extracts from it, which Scaliger had published in his edition of Eusebius in 1606; these however had been made faithfully: nevertheless it must be allowed, that such disjointed fragments could not well enable him to discover how continually Syncellus intermixed his own presumptions along with the quotations which he made from other authors; which rendered it difficult for Petavius to distinguish the real words of Diodorus from the mere suppositions of their meaning according to the opinion of Syncellus himself. But the same excuse cannot be made for the other moderns, who might have consulted the original book of Syncellus published in 1652 by Goar before they wrote.

equally disposed to object to the shorter sum of 1300 approved by Petavius, as to the longer one of 1460; therefore he had no other interest in the cause of accusing the text of Diodorus, than the satisfaction of rendering uncertainty still more uncertain, in order to draw a conclusion against the certainty of all prophane history, which Bolingbroke extended still farther to scripture history likewise.\* But although the duration

\* Marsham beside impeaching the text of Diodorus, accuses likewise the Assyrian period itself whatever its duration was, as being inconsistent with the account of the Assyrian kings in scripture, and still farther he equally objects against the succession of Median kings in Ctesias, which followed the Assyrian period. “Quantum a regibus Assyriorum in ebraicis historiis *discrepat* longissima illa series in Ctesiâ et Eusebio—aut illa Syncelli, quæ a *ratione* et omni *auctoritate* longius discedit? Ob tantas *incertitudines* Ctesianæ Assyriorum *Medorumque* successiones ad scientiam temporum parum conducere videntur.” *Sæc.* 17. Such sweeping accusations, as involve all antiquity in the same condemnation, and do not attempt to distinguish what is true and what is not, may be acceptable to many, but are injurious to literature. On the contrary I have proved, that although the Assyrian period even the shortest is indeed at variance with *reason*, yet it is not with *authority*, but has good *Asiatic* authority for it. But how can this Assyrian period whether true or false be at variance with the account of the Assyrian kings in scripture, when the former

was



duration was uncertain, yet not so the existence of *two successions* of kings in Assyria, as

was ended before any scripture account of the latter begins? They were *successive* and not *cotemporary*, therefore could not be inconsistent with one another. Thus authors run from one error to its opposite: Petavius attempts to confirm *one* portion of scripture by the Assyrian period; while Marsham on the contrary condemns the Assyrian period from its supposed incoherence with *another* portion of scripture concerning Assyrian kings mentioned there, none of whom were in existence until all the former were dead; and this just as well according to the shorter sum of 520 in Herodotus, which he approves, as the longer one of Syncellus, which he condemns. What incoherent reasoning is this! and what follows is little better; for if the Assyrian period of Ctesias was ever so fabulous and *uncertain*, yet it does not follow hence, that the *Median kingdom* in Ctesias, which succeeded to the former, was uncertain likewise. On the contrary both were true so far as respects their existence, and only uncertain in regard to their duration; but I shall prove concerning the latter, that it was in reality the very same succession of kings, as those called in scripture *Assyrian kings*: and although it partakes at its *beginning* of the uncertainty of the duration of the Assyrian kingdom, yet at the *end* of the Median kingdom it agrees with the *date* of the *end* in scripture, and moreover in the *number* of kings, their *possessions* and some of their *actions*; and at the same time it agrees with the æra of Nabonassar in the *duration* of some of those reigns and the *dates* of their commencement, as well as with some other prophane authorities. Condemned as we are through the calamities of former times to form our opinions  
out

as related by Ctesias ; and instead of magnifying with Marsham the degree of uncertainty, it has been my object to discover the cause of it, and hence to collect the real durations out of the different accounts transmitted to us : this cause seems to have been chiefly an ill-founded prejudice against the fidelity of Ctesias ; who on the contrary I find to be himself faithful in the accounts which he communicated to the Greeks, but that it was his informers the Asiatics themselves, who had introduced so much invention and consequently uncertainty into their own history ; the nature and degree of which, when ascertained and removed, will render the prophane history of those *two successions* of kings consonant to scripture history instead of contradicting it.

But although thus far there may be some excuse for Petavius as to his not having rightly estimated the nature of that quotation from Syncellus, to which he appealed, yet no excuse can be made for the *conclusion* which he deduces from the whole of his *proofs* in favor of 1300 years only ; for after having contended, that most ancient authors computed no more than 1300 *from*

out of the scattered fragments of the few books preserved, it is a due distinction between these, which alone gives a claim to judgment.

from Ninus, what is his *conclusion*? It is that therefore we ought to reckon no more than 1300 from Belus. “Quocirca nos cum Eusebio, Justino et Augustino (*Civ. Dei.* 12. 10) ac *plerisque* veterum credimus Assyriorum regnum durasse præter propter 1300 a Belo.” (Vide my p. 178.) Now who are these *plerique veterum*? Justin the earliest of them extant never so much as mentions the name of Belus, but speaks of Ninus as being the first king,\* it is from him therefore only, that he reckons 1300 years. Diodorus also never mentions the name of Belus; but again calls Ninus the first king, and computes in one place 1300 or 1400 years expressly from Ninus: and Petavius himself had but just before affirmed, that *Græci ferme Ninum primum regem faciunt*. Syncellus again as I have shewn supposed Diodorus to reckon 1300 from Ninus; Agathias the same. Sura in Paterculus computed 1300 years to the kingdom, but whether from Belus or Ninus is not known. The computation of Africanus is not quite certain. Cephalion reckoned from Belus, but we are again ignorant of his sum. Castor began apparently with Belus, but reckoned no more than

\* Ninus primus omnium rex.



than 1280; or if he assigned more we are ignorant of the sum. Berosus, Polyhistor and Syncellus made 1460 from Belus, and Ctesias 1360. So that this vast body of *plerique veterum* consists of only Eusebius and his copyist Augustine against five or six more ancient prophane authors, and among these those original historians Ctesias and Berosus; who collected their information immediately from the Asiatic natives themselves, and not from the mutilated catalogues, which had become current in the later age of Eusebius. This *conclusion* therefore stood in such manifest contradiction even to the *proofs* themselves adduced by Petavius, that Marsham, Cumberland and Jackson have deserted him in it, and yet in the very same breath have adopted those proofs, from which this irrational *conclusion* was deduced; the insolidity of which ought to have led them to examine better the *premises* on which it was founded: and by their neglect of this I have been forced thus to trace back their errors to the testimonies adduced so long ago by Petavius, in order to discover on what foundation their opinions rested; which we have found to be only on errors, misrepresentations and conjectures. Yet so great was the influence of system over Petavius himself in forcing  
 Assyrian

Assyrian sums to harmonize with the computations of the Hebrew text ; and so great also has been his learned authority over others ever since, as to render both himself and his followers blind to the most evident truths, which opposed their several opinions and systems, although these are often in direct opposition to one another. Upon the whole, Ctesias was among the first who communicated to the Greeks some information of that astronomic chronology of the Asiatic nations concerning the Assyrian kingdom, of which he probably had never been able to obtain quite a perfect account among the Persians ; but which was within a century afterwards communicated with accuracy by Berosus. But Ctesias appears to have given faithfully the best account, which he had been able to collect ; and for the extravagance of this he was no more answerable, than Sir W. Jones in relating the nature of another such astronomic chronology among the modern Hindoos, so similar to that of the Chaldeans concerning Assyria ; which method the Hindoos had not improbably borrowed from the Chaldeans of old. Petavius wished to find in this account some confirmation of scriptural computations in the earliest antiquities of the Jews according to the Hebrew text ;

and having taken a decided opinion to this purport, he attempted to pull down every thing which opposed what he had in view to build upon this foundation. But Marsham took a different road, for being too much inclined to doubt concerning prophane antiquities, he rather aimed to pull down every thing, than to build up any thing: yet upon many of his false maxims it was, that Newton afterwards erected a new system, the effect of which would be to put a total end to all ancient chronology; but in which however he did not always comprehend Marsham rightly, of which I will give one example in my *Illustrations*, thus while Marsham did not take the necessary steps to discover any truths, Newton took a sure method to destroy them all.

*Enquiry for what Purpose, in what Age and by what Persons the Assyrian Period of 1460, as communicated by Berosus, was mutilated and reduced to 1300 Years; which we shall find to have been not done, until after the Time of Eratosthenes, near a Century after Alexander, and by such Persons, as had adopted the 407 Years of Eratosthenes for the Interval, from the Capture of Troy to the first Olympiad.*

THE



THE refutation of the foregoing objections by different persons to the integrity of the present text of Diodorus, has necessarily led us into a long discussion of them, both in order to explain what Diodorus may possibly have *meant*, and also what Syncellus has actually *said* concerning the Assyrian sums in that historian; the crabbed and disjointed reasoning moreover of Syncellus required of us to compare almost the whole of what he has said, before we could be certain of his meaning in any one particular sentence, and it was for want of this, that former authors have put erroneous constructions upon some of his words: but after our having at first ascertained from Berosus and Polyhistor the originality of the period of 1460, and having also in the preceding section confirmed from Ctesias and Diodorus, that it was estimated at 1360 at least by those authors 200 years and even 600 before Africanus, all that is still wanting to the enquiry is to point out the probable cause of its being afterwards reduced to 1300. Now as no express information concerning this has been transmitted to us by the ancient authors still extant, we can only collect the cause and time of the reduction out of the nature and circumstances of the

mutilations themselves, and of the necessary effects produced by them; and thus we may with propriety reason backward from the effects produced to the probable objects in view by those who made the mutilations. The first of these circumstances is, that we have the testimony of Syncellus the latest writer on the subject, to add to the other evidence already adduced above, that it was only such late authors as Castor, Cephalion and Eusebius after them, who had reduced the period to less than 1300 from Ninus, (or at most 1306 as in Diodorus) which reductions he condemns, as well as the sum of Diodorus;\* this indicates, that all such reductions must have been apparently made  
later

\* “Cephalion pluribus annis minor est quam Diodorus, et aliquid absurdi colligitur ex uno vel altero eorum, ne dicam ex utrisque; Diodorus enim magis ad veritatem accedit, dicens 1300 annos et plus totum Assyriorum imperium a *Nino* durasse—in Castore autem Assyriorum regibus sunt anni 1280, quem Eusebius *potius* sequitur.” *P.* 168. This is the sentence to which I referred in note to p. 192. Now the unknown sum of Cephalion, the 1280 of Castor and the 1240 of Eusebius from Ninus may be all indeed less than the 1305 of Diodorus from Ninus; yet as even Eusebius made 1305 from Belus, this proves indirectly that Diodorus could not have made less than 1360 from Belus, and that this sum in his text must be right, whether reckoned from Belus or not.

*later* than the age of Eratosthenes: and it is farther confirmed by the object in view of those who made them, which is discovered to us from their being all found accommodated to the sum of 407 years or less of interval between Troy and the Olympiads, the very sums first introduced by Eratosthenes. A second circumstance is, that the omission of the 4 Assyrian reigns of 162 years is not made until after the end of the reign of Teutæus the 28th king; and a third circumstance is, that the omission is made immediately after that 28th king: these facts form a presumption, that it might be made for some purpose relative to the interval from Troy to the Olympiads; and if it shall appear farther, that 162 years was the precise sum, which was required, or very nearly, in order to reduce that interval from the Assyrian date of the *middle* age to the 407 of the *later* age of Eratosthenes, we may then not unreasonably conclude, that this was the actual purpose for which that omission was made. To these circumstances we may add moreover that the name of *Teutæus* is more similar to that of *Teutamus* than of any other king, and that under him the ancients before Diodorus had according to his account fixed the Trojan war; the



1000 years also assigned by them to the Assyrian kingdom before that war, would be expired within a few years after that 28th king, though not sooner, and the interval from thence to the first Olympiad would exceed 405 years by those 162 years; all these things together (I say) give foundation for a reasonable conclusion, that they were in some *later* age than Eratosthenes purposely omitted, in order to bring down the *end* of the reign of Teutamius or Teutæus to be cotemporary with the capture of Troy in the 405th year before the first Olympiad. Now, in order to ascertain better the necessity under these circumstances for such an omission of those 162 years, it must be observed, that beside those omitted 162 immediately after Teutæus and the Trojan war, there still remained 314 more according to the catalogue of Berosus down to the end of the kingdom; and about 40 more according to the *historic* account of Herodotus and others in the *middle* age from the end of the kingdom to the Olympiads, i. e. 516 in all or nearly from Troy to the first Olympiad: this then is the very sum, which we have all along found assigned to that interval by the Greeks of the *middle* age, viz. from 508 to 520 years;

years\*; and it shews again that in the middle age the 28th king Teutæus or Teutamius must have been considered as cotemporary with the Trojan war, so as that it *began* under him, although it was not *ended* until the reign of his successor; and the middle age was that of Ctesias†. Such a tradition thus  
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\* Censorinus has preserved another testimony to this current computation of the middle age; for he says "that Aretes reckoned that interval at 514 years." (C. 21.) Who this Aretes was or when he lived is quite unknown; possibly the name ought to be Crates the grammarian, whom we find mentioned by Eusebius and others as a computer of the time when Homer lived, which would in course lead him to fix the Trojan war: he was a cotemporary and opponent to Aristarchus, and as he is said to have placed Homer *before* the return of the Heraclidæ, he might probably prefer the old and longer computation of the middle age to the later and shorter one of Eratosthenes. At any rate Lydiat, Marsham and others have too rashly attempted to correct the text of Censorius here, by reading either 414 or 434 instead of 514. Vid. Marm. Arundel. by Mattaire p. 302, in note lin. penult. and p. 251. Thus critics go on to destroy the few remains of ancient evidence!

† I may here add, a farther proof, that Diodorus by *ἐκ Τροίας ἀρπάγευσαι* meant the beginning of the expedition, not the end of it, for other testimonies in like manner are found to refer to the commencement, as well as to the end: thus for instance both of those events are found fixed to their respective dates even in so short a chronology as that of the Parian marble; and the commencement  
of

once established the adopters of the reduced sum of 407 by Eratosthenes might be desirous to preserve afterwards; but these 407 years only would bring down the Trojan war a whole century *after* the 28th king Teutæus; and another variation, made likewise after the time of Berosus, would require a greater amputation than of these 100 years. For instead of the former *historic* interval of 40 years after the end of the kingdom, Berosus a little before Eratosthenes had substituted 91 years, agreeably to the *astronomic* chronology of Asia; and the credit of this recent history of Asia by Berosus not improbably caused his longer interval of 91 to be preferred afterwards to the historic 40 of the more ancient Greek writers. We are at least certain, that it had been introduced into Greek computations by its being found subsisting in the total sum of 307 by Polyhistor, and also in the old Latin chronology referred to before from Scaliger, which was almost a translation from Africanus; for it says, “Ab istis (sc. regibus Assyriorum) in primum Olympiada  
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of the war is there expressed also in the very same phrase as in Diodorus Αφ' ου οι Έλληνες εις Τροίαν εστρατευσαν. The capture follows afterwards at 10 years distance, Αφ' ου Τρόια ήλω. Both dates were apparently copied from the history of Ephorus.



anni 67:" now it must be observed, that this system by Africanus had introduced an apocryphal second Ninus as an Assyrian king after Sardanapalus reigning 19 years; which added to 67 make up 86 years, so that he had at least greatly exceeded the former 40 years\*. But thus the 4 first reigns of 162 immediately after the 28th king, if added to the 314 of the subsequent reigns, and to the 91 years of interval after them

VOL. III.

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\* These sums in Africanus might possibly be the cause of Orosius reckoning 64 years, he not having taken into account those other 19 years of the second suppositious Ninus as part of the interval in question: but no ancient author has made so many as the 100 years of interval adopted by Petavius, which is entirely conjectural and imaginary; except that Justin has indeed assigned 131 years of interval, an error into which he was possibly led, by not rightly comprehending the computations in Diodorus, as I shall afterwards shew. So that the 91 of Polyhistor is the utmost extent assignable to the interval, for which any sufficient evidence is preserved; and that this was the sum of Berosus appears from Polyhistor computing 307 years from the end of the Assyrian kingdom to the accession of Cyrus: for this accession being placed by the ancients on 1st of 55th Ol. the 216 Olympic years down to this, if deducted from 307 leave 91 years for the interval before the first Olympiad; which fixes the end of the Assyrian kingdom on the 120th before Nabonassar. See my vol. i. p. 342. Syncellus proves that Polyhistor was in this subject a mere copiest of Berosus.

down to the first Olympiad, would amount to 567 years; therefore instead of an omission of only one century, there would now be required an omission of all those 4 first reigns of 162 years, in order to reduce 567 to the new period of 405 as computed *after* Eratosthenes. Thus then we find, that by an omission of those 162 years the 28th king Teutæus would be brought down 162 years, so as to be cotemporary as before in the middle age with the Trojan war at 405 or 406 years only before the Olympiads; which were the approved sums in the time of Eusebius and apparently deduced from Eratosthenes\*. Since therefore the effects in course produced by such an omission of

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\* "A captivate Troiæ usque in primum Olympiadem fiunt anni 400 alias 406." *Euseb. ap. 1m. Ol.* He himself makes up 406, but it is because he reckons 315 to the reigns of all the kings after the 28th, exclusive of the first 4 of 162; whereas Berosus makes only 314 of them, consequently 405 would be the whole interval. Eusebius in his *Præp. Ev.* mentions 408 as the sum of Eratosthenes, but Clemens. Alex. makes only 407, as also does Censorinus. These variations seem to have arisen partly from the different methods of reckoning inclusively or exclusively of the first and last year; and partly because Eusebius computes also the interval at 43 instead of 40, which lengthens his sum; and such variations as these made by other writers help to account for their different reckonings from 400 to 408.

162 years tally so exactly to what I have supposed to have been the cause of it, and to the necessity of the case in order to reduce the date of the war in the middle age to 405 in the Assyrian period, as it had been before in the Greek computation, I think, that we may not unreasonably conclude, that this was the actual cause of that omission : for it was not possible by any other method to preserve the received tradition of the Trojan war being cotemporary with Teutamus or Teutæus, and yet to bring it down so late as to 405 years only before the first Olympiad. On this account Syncellus, as we have seen, gave up the cause of that ancient tradition, and by not omitting those 162 years brought down the war to the 32d instead of the 28th king, that is to Babius ; yet at the same time so very unwilling was he to quit that tradition altogether, that he falsified the name, and pretended, after some others, that Babius possessed the secondary name of Tautanes, in order to come as near to the name of Teutamus as he could conveniently. Thus then the queries at first proposed are answered concerning the age, the purpose in view and the persons, who were interested in making such an omission ; and this we find to have been apparently done by the best



of the prophane chronologers, namely, those who adopted the system of Eratosthenes; which is a sufficient apology for its having been adopted after them by Eusebius likewise: but it was only by mere accident that it happened at the same time to be most in harmony with the computations of the Deluge and Nimrod in the Hebrew text adhered to by Eusebius; and this accidental coincidence of scripture with the mutilated Assyrian sum was the chief foundation for the supposed identity of Belus and Nimrod.

But although it thus appears, that the 28th king Teutæus must have been the same as the Teutamus in Diodorus, and the real king under whom ancient tradition and the computations of the *middle* age had placed the Trojan war, yet still a question arises, how that war in the *later* ages of Eusebius and Syncellus came to be considered as happening under the preceding 27th reign of Tautanes; notwithstanding that it ought apparently to have been placed under a *later* rather than an *earlier* king, if the total interval after Troy was reduced from about 508 to 405. Now the origin and cause of this also we shall be able to discover, if we attend to another variation found in these computations of both Eusebius and Syncellus. For I have shewn, that origi-  
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nally the first Greek chronologers after Eratosthenes, who made the omission of 162 years, must have adopted at the same time the *astronomic* interval of 91 years, as stated by Berosus, from the Assyrian kingdom to the Olympiads; as appears by the exact coincidence of their sum 405 with that supposition, after omitting the 162; and also by the testimony of Polyhistor to the total sum of 307 years down to Cyrus: but we are certain, that both Eusebius and Syncellus preferred the shorter *historic* interval of about 40 years current at first, which in course would cut off their difference, i. e. 51 years from the interval of 405 after Troy. In this they might probably follow Castor or Cephalion, or some other profane author, who might have discovered good evidence for rejecting that longer *astronomic* interval of 91 by Berosus, and receiving once more the *historic* shorter interval of 40 from the earlier Greek historians Hellanicus, Herodotus or others\*. But the reception

\* That both Hellanicus and Herodotus had written an account of Assyrian history appears by the following words, which Syncellus has quoted out of Cephalion. “ Quam incongruè de temporibus et regibus Assyriorum historici Græci scripserint præclarus Cephalion declaret, sic dicens—  
*Scribere aggredior res illas de quibus alii memoriam egerunt,*

reception again of those 40 years would necessarily produce a new variation of reign  
for

*egerunt, imprimis Hellanicus Lesbii et Ctesias et Herodotus; quod olim in Asia regnaverunt Assyrii, primum quidem Ninus Beli filius—dein de subjungit, Semiramidis natalia et annos 52 regis Nini—post hæc dicit, quod Semiramis Babylonem manibus sic instauravit, ut a plerisque dicitur, scilicet a Zenone et Ctesia et Herodoto et aliis post illos, &c."* P. 167. Now Herodotus could have never been here enumerated along with Ctesias, Hellanicus and others, as an historian of Assyria on account of the little, which he has said of that kingdom in his history now extant; in which he has only once mentioned the names of Belus and Ninus, and even this only in his History of Lydia, and moreover in so brief a way, that it is doubted by many whether he meant the kings of Assyria or some other persons of those names: besides those names he says nothing more of Assyria, than that its kingdom lasted 520 years, before it was dissolved by the Medes; that Semiramis had only embanked the river Euphrates, and another queen Nitocris after that destruction by the Medes had built the walls of Babylon and that he proposed to publish a particular history of Assyria. It must have been to this particular history that Cephallion referred in the above passage concerning the testimony of Herodotus relative to Assyrian history; and this reference to Herodotus for that purpose becomes an additional evidence to those adduced by other moderns, that Herodotus did actually publish his particular *History of Assyria*, as he proposes in his work now extant, which was, as thus appears, existing in the reign of Adrian.—As to Hellanicus, it appears likewise by the above quotation from Cephallion, that he must have written some account of  
Assyrian



for the capture of Troy from that of the 28th king Teutæus, and force it upward 51 years into the preceding 27th reign of Tautanes; in order to preserve the full sum of 405 after it, by making this sum commence 51 years *sooner*, and thus compensate for the 51 years so cut off from the interval *after* the end of the Assyrian kingdom; that

Assyrian affairs even before Herodotus; for that it is the ancient well-known Hellanicus, who is there referred to, is evident by his being styled *Lesbius*; there being another later historian of the name of Hellanicus, but not styled *Lesbius*. The historic work of the former was intitled Κτισεις εθνων και πολεων—*The Foundations of Nations and Cities*, several portions of which are quoted by antient authors under the titles of *Troica*, *Persica*, *Ægyptiaca*, *Lydiaca*, &c. and there must have been another portion containing *Assyriaca*. What other historians beside Hellanicus and Zeno before Herodotus had given some account of Assyrian history we are not informed, but there may have been others, since Cephalion says, that he compiled his Assyrian history out of 570 different authors, as Photius has mentioned *cod.* 68. It must be extraordinary if neither Herodotus, who mentions 520 years as the duration of the Assyrian kingdom, nor any of those other antient historians of the middle age, should have noticed how long that kingdom perished before the first Olympiad or some other well known event in Greek history. It was therefore doubtless from some of them, before Berossus and Alexander, that those later chronologers, whom Eusebius copied after, revived that *historic* interval of about 40 years only, which as I have shewn agrees with scripture.

that is, instead of 405 being now made up out of 314 and 91, it would be made up of 51 before 314 and 40, equal to 405. This variation was a necessary consequence of the other, in case the full sum of 405 years was still preserved; and hence apparently was the origin of the capture of Troy being fixed by Eusebius and Syncellus so early as the 27th reign, whereby the name of Tautanes came to supplant that of Teutæus or Teutamius as the king cotemporary with the Trojan war, that war being through these means forced upwards into the preceding 27th reign\*. But this necessarily occasioned still another variation, which is, that by the capture of Troy being thus placed a reign too soon, there would be only 941 years from Belus to this event instead of 1000, even although Eusebius allowed 65 to Belus instead of 55. Now this capital variation from the more antient statement, as given by Diodorus, does of itself alone sufficiently shew, that the original

\* Agreeably to this Eusebius places the capture of Troy on the 8th year before the end of Tautanes the 27th king, to which on adding the 40 of his successor Teutæus the 28th, they amount to 48, and these *before* the end of Teutæus compensate for the 48 taken from 91 *after* the end of the kingdom, those 91 being by him reduced to 43; thus 48 and 315 and 43 still make up his 406 instead of 315 and 91.

computations have been disturbed by these later chronologers, both before Troy and after; but at the same time it again confirms, that the capture of Troy was at first or by those of the *middle* age not fixed sooner than Teutæus the 28th king, that he was the real Teutamius referred to by Diodorus, and this not improbably copied from Ctesias. So that here again as before, the very nature of the discordances themselves, produced by the variations of the *later* chronologers among the ancients, leads us, as it were by hand, to discern the original state of these Assyrian computations in the *middle* age, before so many variations had been introduced; and at the same time points out the motives and causes of them. Hence it appears, that their fault has been only the same, which, as I have shewn, affected likewise Usher, Petavius and other modern system-makers, that is, an attempt to make truth and fable unite amicably together; but hereby while they removed one error, they introduced some other in another place: for being desirous to adopt the shorter and two *truer* intervals of 405 and 40 years, they attempted to accommodate these later truths to the former *fabulous* astronomic chronology of 1460 among the Assyrians. But in doing



this they entirely disturbed the former harmony of it by the variations which they were hence forced to introduce; and it is these later variations thus intermixed with the relics of the more ancient system, which produce that confusion and incoherence in the subject, of which all authors have been sensible, yet without rightly discerning the origin of their complaint; whereby they have been tempted to make the case still worse by the very remedy they have applied, that is, by their conjectural alterations of the Assyrian sums in the text of different authors. For the original statement of the *middle* age, which Diodorus has pointed out to us, is all in harmony, namely 1460 for the whole period, 1405 after Ninus, 1360 after the erection of Niniveh, 1000 down to the *capture* of Troy *soon after* the 28th king Teutæus or Teutamius; which leave 460 to the end of the kingdom, and about 40 more from that event to the 1st Olympiad make up the 500 years of the middle age from Troy to the Olympiads: but as Herodotus made it amount to 508 or a few more, he might possibly reckon his sum from the *beginning* of the expedition against Troy, not its *end*, just as Diodorus did; and in fact his phrase is like that of Diodorus, not a *capta*  
Troia

*Troia but a bello Trojano.* On the other hand such of the middle age, as made 518 or 520 years of the period after Troy to the Olympiads, might in this sum allow 8 or 10 years for that interval between the capture of Troy and the birth of Pan from Penelope and Mercury, which Herodotus has left undetermined, as may be seen at my p. 70 and 72: or else they might have reckoned from 40 to 50 years between the end of the kingdom and the 1st Olympiad, as accordingly Eusebius has made at least more than 40, though Syncellus does not, and the sum in Herodotus is uncertain.\* — Upon the whole then we

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have

\* I have not observed, that any other ancient author except Herodotus has noticed this fable of the procreation of Pan by Mercury and Penelope; now as Penelope has been always mentioned by the ancients as a pattern of chastity, possibly this tale may have arisen only from a figurative mode of expression in giving to Ulysses himself the name of Mercury; Ulysses having always had the character of dealing in the art of persuasion like Mercury: and as he is said to have been long in his return home from Troy, Pan might thus be his own son after his return. So that the conjecture of Larcher at my p. 72 may be nearly right, that we ought to allow about 10 years for the interval in question, as had been indeed proposed by others before him; who seem to have had no other evidence for this sum, than that Penelope was at least chaste during those 10 years till Ulysses returned.

have found, that all the confusion and incoherence subsisting at present in Diodorus, Eusebius, Syncellus and other ancients concerning any of the sums relative to the Assyrian kingdom, or the interval after it, or the date of the Trojan war or the name of the king under whom it happened; seem to have neither arisen from any corruptions made in the text of those authors in later ages, nor yet from merely arbitrary or accidental variations among the original authors themselves: but may be traced to one general cause, that is, to the different computations and consequent reductions made by the *later* Greeks in the chronology of their own antiquities relative to the capture of Troy and other events. These variations either the same or else other authors appear to have extended also to the Assyrian chronology, and to have thus altered it in conformity to the above-mentioned variations in the chronology of the *later* Greeks; and hence in particular is the cause of that omission of 4 whole Assyrian reigns at once containing 162 years: which we have found to be the exact reduction necessary to fix the end of the 28th king Teutamius or Teutæus at only 405 years before the 1st Olympiad, agreeably to the computation of Eratosthenes

or



or other Greeks of his *later* age. But at the same time we have found the magnitude of this reduction to be so great, that it supplies us with an *additional* evidence to those others mentioned before, of the following fact; that not less than 1460 years or at least some larger sum than 1300 must have been well *known* in the age of Eratosthenes to have been the sum then currently received as the total duration of the Assyrian kingdom, 200 years before Diodorus and 400 before Africanus: otherwise they could have never made so great an allowance as 1000 years before the Trojan war, and still retained 405 more down to the 1st Olympiad including the interval after the kingdom; notwithstanding that they cut off or omitted 162 other years in the middle of the computation. This then is an *internal* evidence of the originality of the sum 1460, which confirms the *external* ones adduced at first from Berosus, Polyhistor, Diodorus and Syncellus.\*

The

\* There were other ancients however, who reduced the interval after the capture of Troy to a still less sum than the 405 or 407 of Eratosthenes. Thus Censorinus informs us, that Sosibius, who lived before Eratosthenes, had reduced it to 395, and some others who lived before Strabo and Pausanias must have reduced it to 387 and 360. There was certainly

The above *history* of the variations,  
made by ancient chronologers in the total  
duration

tainly some room to go thus far, though not so far as with Newton to 128 years only before the Olympiads. For it appears from Clemens, that Eratosthenes allowed 80 years to the two first generations of Orestes and his son Tisamenus ending with the return of the Heraclidæ; and 60 more to the two next generations of Melanthus and Codrus between the return of the Heraclidæ and the Ionic migration: this allowance of 140 years to both together for only four generations was certainly a large one; accordingly we find in Strabo and Pausanias, that each of these two intervals were reduced in their age to 60 years, i. e. 120 for both, which would reduce the total after Troy to 387 or 374. We find moreover from Appian, that others had reduced it to 360: for he informs us, "that, when Fimbria the Roman general destroyed Ilium in the 3d year of 173d Olympiad, some persons computed this to have been 1050 years after the former destruction under Agamemnon." *Mithidat.* Now in 172 Olympiads and two years are contained 690 Olympic years, which deducted from 1050 leave only 360 from Troy to the Olympiads.—Nevertheless there is still something to be said in favour of the longer reckoning by Eratosthenes of 140 to the two first intervals; for it does not clearly appear, whether there were not in reality three generations between the return of the Heraclidæ and the Ionic emigrations, i. e. five in the two intervals, for which 140 years might not be too many, and Pausanias himself affords evidence for this: yet Newton has not adverted to this circumstance, although he himself has allowed 80 years to that first interval; which shews the incoherence of his system, as thus there remain only 48 from the Heraclidæ to the Olympiads.

duration of the Assyrian kingdom or of particular portions of it, will discover to us moreover the cause of another fact, which seems to have greatly perplexed Cumberland; and I think, that I may without impropriety call it a history, although it is not altogether compiled out of historic accounts still preserved in ancient authors concerning the reasons for making those variations: for neither is it the produce of arbitrary invention, but is chiefly deduced from internal evidence contained in those very variations themselves; the nature of which do of themselves almost as clearly proclaim the causes, which gave rise to them, as if they had been found expressly related in some ancient historian. Now a difficulty, which much perplexed Cumberland is this—How it came to pass, that while some ancients reckoned 1300 or 1305 years from Ninus, others should reckon no more than the very same sums from the prior reign of Belus.\* This agreement,

\* “ I may shew, that there is an agreement between those, who begin the empire with Belus, and those who begin it with Ninus—this may *seem strange*, but I will prove, that such an agreement is, and then offer a reason for it—those that begin with Belus assign but 1300 years as in Eusebius and Augustine; those who begin with Ninus, as Diodorus, make



*agreement*, as Cumberland calls it, that is, in the sum, for it is rather a disagreement in computation, did not arise from the imaginary reason which he afterwards assigns; but merely from the two different sums made known to Greece by Ctesias and Berossus in two different ages: for those who had at first adopted the 1360 of Ctesias and understood them to mean the total sum after Belus, could not reckon less than 1300 or 1305 from Ninus, after deducting the 60 or 55 of Belus; and this is the computation, which Syncellus ascribes to Diodorus. But other authors, who lived later than Berossus, and preferred his longer sum of 1460, found it impossible to adopt at the same time the reduced 405 years of Eratosthenes after Troy, unless they either shortened those 1460 Assyrian years by full 160 years after Troy, as I have shewn, that they actually did; or else they must have departed from the then current tradition in Greece established in or before the *middle* age, that the Trojan war was contemporary with the Assyrian king Teutamus or Teutæus, which tradition however they preferred to preserve; but such a defalcation

make about the same number—notwithstanding the difference of those beginnings." *Cumberl.* p. 192, 193, 195.

falcation of 162 years near the *end* necessarily reduced the total period to 1300 years only after Belus, just as it was before reduced to 1300 after Ninus by the deficiency of 100 years in the *total* duration of the 1360 by Ctesias. This sum therefore, of 1300 in neither of the two computations was the original one as not including the whole; for as to that of Ctesias, if his 1360 years was really meant by him to be reckoned from Belus, it is probable, that he had not been able to procure among the Persians so long afterwards a true and perfect account of the original Assyrian period: and this accidental deviation from it by that historian through want of information, together with the intentional mutilation of the longer sum of Berosus by the later Greeks, brought the two computations to agree in quantity although beginning with different kings. Hereby we discover both the cause of the difference in what they differ, and also of their agreement in what they agree; and find this latter to have been merely accidental, owing to the following circumstance, that 1360 when reduced by the 60 years of Belus would want 160 of the true sum 1460 at the *beginning* of the period; just as the omission of 160 at the middle and *end* of it would

again reduce 1460 to 1300 : which omission was owing to a cause totally unconnected with the former case, that is, to the necessity of bringing down the end of the reign of Teutamius to be only 405 years before the Olympiads. This then explains the origin of what Cumberland thought so *strange*, and which he has accounted for as strangely, although it was owing to a very simple cause and that merely an accidental one.

There was however still a third class of chronologers among the ancients, who neither adhered to the sum of Ctesias, nor yet to that of Berosus when thus reduced by 160 years; and these seem to have proceeded in their computations upon quite a different principle from any of the former reducers of the Assyrian period : for instead of adopting either the whole *astronomic* period of Berosus or any of the reductions of it, they appear to have rather augmented the *historic* sum of 520 in Herodotus; and to have taken this for the foundation of their computation of the duration of Assyrian dominion. Thus we find the sum of 1070 years only to be mentioned in Paterculus as one computation by this class of authors; and Cephalion is said by some of the moderns to have reckoned



reckoned only 1000 years in all for the Assyrian kingdom : this last however is not quite certain, as it depends only on a passage in Syncellus, where his text is in part so corrupt, and in part so defective, that no certain conclusion can be drawn from it. This third class of ancient authors then might possibly have collected some historic evidence or traditions for the existence of an Assyrian state before the commencement of Belus and of those 520 years reckoned by Herodotus from the accession of that monarch. We have certainly some of these traditions still preserved in Syncellus from Polyhistor [see my p. 152] and Diodorus likewise allows, that many other kings and kingdoms had subsisted in Asia and even at Babylon long before Ninus; together with considerable cities both in the province of Babylon and elsewhere, although Babylon itself was not built until afterwards.\* Hence then probably it was, that

\* “ *Antiquitus reges indigenas, quorum nec facta inclaruere, nec nominis memoria durat, Asia habebat : primus inter eos, quorum historiae memoriam propagarunt Ninus res magnas peregit—Babylon quæ nunc est, illo tempore nondum erat condita; sed alias Babylonia urbes non contemnendas habebat, et ejus regem Debellatum Ninus interemit.*”

*Lib. 2.* Justin writes to the same purport “ *Principio*

that the ancient chronologers in question carried back Assyrian antiquities to 1000 or 1070 years instead of the 520 only which Herodotus had reckoned down from Belus; agreeably to this we find, that Syncellus after copying from Polyhistor some accounts of these prior kings, supposes Nimrod to be the first of these kings called *Evechous*, whose reign began according to the catalogue of Polyhistor 440 years before Belus; which however would make the total to be only 960 out of the 1070: but Jackson, who agrees with Syncellus, that Nimrod was *Evechous*, places his accession at 567 before Belus, which would make the total of the Assyrian state to have been 1087 years. But these computations founded upon doubtful traditions are altogether uncertain, and only serve to shew it to be not improbable, that this third class of chronologers among the ancients, who confined the Assyrian period to 1000 years more or less, might have taken such *historic* traditions for the foundation of their calculations and have *added* to the 520 years of Herodotus, rather than reduced the longer  
*astronomic*

cipio nationum imperium penes *reges* erat—primus omnium Ninus veterem gentibus morem nova imperii cupiditate mutavit; his primus intulit bella finitimis." C. 1.

*astronomic* period of 1460 by the Asiatic sages. These 567 years however, unless they be prefixed to the *astronomic* period of 1300, not to the 520 of Herodotus, will not go back sufficiently into antiquity for the real time of the existence of Nimrod, and they are perhaps intitled to the more credit because they do not: but Syncellus, after Polyhistor enumerates several other kings, who were prior even to Evechous; and it must have been some one of the first of these, who was the same as Nimrod, in case there be any identity whatever between him and any profane king so recorded. This however is again so very uncertain as indeed to be not worth mentioning, except in order to point out to readers the very precarious nature of all such pretended identities with Nimrod as modern chronologers and commentators have contended for; in order to ascertain which the better, they have not scrupled to turn the text of several prophane historians upside down, in accommodation to their different opinions; and thus to have written only learned romances instead of scriptural expositions. Yet thus far indeed may be true, that it is only this third class of ancient chronologers concerning Assyrian history, who can any way with truth approximate to the computations



tations in the Jewish scripture, or in any respect be considered as being in harmony with it, although indeed they may not possibly go back so far into antiquity. Usher therefore and Bossuet have rightly dismissed all other Assyrian accounts except these as being altogether fabulous, and not worthy of being mentioned in connexion with sacred history; notwithstanding the pretended evidence and unsolid arguments, which Petavius and others had before the time of those authors collected, in order to support the plausibility of such learned tales: and it would have been well for both prophane and sacred history, if later authors had not revived those over zealous reveries of Petavius, which have however been too inconsiderately adopted by Calvisius, Jac. Capellus, G. Vossius, Helvicus, Cumberland, Jackson and many others; so very detrimental to truth and literature is the authority of able writers when once established, although built at first upon ever so weak a foundation!\* We can find then  
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\* “ About the time of Deborah [i. e. at the time of the Argonautic expedition 80 years before the capture of Troy] we may place the foundation of the Assyrian kingdom under Ninus, which he established at Niniveh, a more ancient city however, which had *long before* become celebrated, although

no sufficient foundation for the extension of the Assyrian kingdom by Ninus until *after* the Trojan war, and possibly in consequence of the dissolution of the prior empire over Asia formed by Memnon but dissolved through his death at Troy; out of the relics of which that under Ninus seems to have been at first composed: in case there be any truth whatever in those Asiatic traditions, which the Greeks have transmitted to us; and which are at worst consistent at least in the manner that the Greeks have transmitted them, but have been rendered incoherent by the imprudent zeal of Christian expositors of scripture, while they attempted to connect such later events with the more ancient Jewish antiquities soon after the dispersion at Babel; so that they have

though rendered still more illustrious by Ninus. Those, who allow 1300 years to the Assyrian kingdom, proceed therefore on a good foundation, in case they mean the antiquity of that city [said in scripture to have been built by Assur] while Herodotus, who allows only 500 to it, may speak solely of the extension of the *Assyrian empire over Asia* by Ninus in later times." *Bossuet on Univ. Hisotry*. Here we find, that Bossuet has fixed Ninus nearly at the date, at which I have found Belus to have begun his reign, who is allowed by all the ancients to have been the founder of the Assyrian empire in Asia, although his son enlarged it by his conquests.

have made the scripture history of doubtful authority through their expositions.\* On the contrary those expositors, who place Nimrod and Assur at the head of a set of unknown kings of Assyria 800 years more or less before Belus, are the only ones, who in reality connect sacred history in any degree of harmony with profane accounts; and although they may not indeed be quite accurate with respect to the dates of such early events, yet at least they are not in contradiction to profane traditions or to scripture.

Lastly, although I have ascertained above what were the current dates of the capture of Troy, both according to Herodotus, Ctesias and others of the *middle* age in Greece, and also according to the reduced dates of that event in the *later* ages of Ephorus, Eratosthenes and others; yet hitherto I have said nothing concerning the  
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\* When I said at p. 160 that Homer had mentioned nothing of Memnon, I meant, not in his *Iliad*, nor any thing any where of his having been deputed to the siege of Troy by any king of Assyria: but in his *Odyssey* he does in one line mention Aurora's son as having been present at that siege, who is supposed to be Memnon; and in the descent *ad inferos*, he mentions Memnon by name as being one of the deceased there, but this is the whole.



computations of the *earliest* Greek historians, Hecataeus, Hellanicus or others before Herodotus. This I omitted because it did not necessarily relate to our subject, nevertheless it may be now acceptable to readers to have the account of the Greek computations in different ages completed: and indeed it will be thus far useful to ascertain also those of the earliest Greeks, as it will help to abate our wonder at the great spirit of reduction in Greek antiquities, which prevailed among authors in the *later* age of Alexander; when it shall be seen to what extravagance the *earliest* Greeks had carried their computations, and also how little reformation had been made in them during the *middle* age. We shall also at the same time be less disposed to surprize at the bold reductions and different variations made in the chronology of Assyria, when we perceive, that the very same had taken place before in the antiquities of the Greeks themselves, so that they kept pace with and naturally accompanied one another in reformation, just as they had done before in extravagance. The computations of the earliest Greek historians had indeed been so little reformed by those of the middle age, that Ephorus and his followers after Alexander were under the necessity to cut

off a whole century, and a similar reduction of the Assyrian period followed in course:\* I know however of only one positive

\* Ephorus was the first Greek historian, so far as any accounts have been preserved, who attempted to cut off above a whole century from the computations of the *middle* Greeks concerning the Trojan war; but it has so happened, that his successors Timæus and Eratosthenes have obtained all the credit of it; yet that Ephorus had anticipated them in this reduction clearly appears, however little it may be known, from one of his computations, which Diodorus has preserved, viz. “ that Ephorus had reckoned only 750 years from the Heraclidæ to the siege of Perinthus by Phillip,” at which event his history ended. (*Lib.* 16.) Now this siege was in the 4th of Ol. 109, which 109 Olympiads being equal to 436 years, these deducted from 750 leave only 314 years from the return of the Heraclidæ to 1st Ol.; whereas Eratosthenes afterwards extended this interval to 328, as appears in Clemens. If then to those 314 we prefix the usual 80 from Troy to the Heraclidæ, the total interval will amount to no more than 394, which is the very sum, or within one of what Censorinus attributes to Sosibius; who lived later than Ephorus, but before Eratosthenes, and might not improbably have founded his computation upon those 314 of Ephorus. Ephorus however himself may have made more of that interval; for if he prefixed 100 years for the first interval from Troy to the Heraclidæ, his sum would amount to 414; which however is still less than the 417 of Timæus. But if he prefixed 120 for the first interval, the total would be 434, which is the very sum in the Parian marble, and which therefore may have been either the very sum of Ephorus, or at least

positive testimony now extant concerning the sum of the earliest Greeks relative to the Trojan war, and this has been preserved by Clemens, viz. “ that Duris reckoned

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least founded upon his interval of 314. Now the commentators on that marble express some surprise at the date 434, of which they seem to think no traces are to be found any where else; and yet we see, that it is the sum necessarily arising from the first interval of 120 being prefixed to the subsequent interval of 314 as stated by Ephorus: it is therefore not improbable, that it was founded on and derived from that computation of Ephorus. For not any one of the three sums, which Clemens enumerates as current ones for the first interval i. e. 120, or 100, or 80 (*Strom.* i. p. 403) will form 434 if prefixed to the 327 of Eratosthenes or to any other received sum except the 314 of Ephorus: that marble was also inscribed before Eratosthenes wrote and possibly before the *Olympionics* of Timæus were composed, yet later than Ephorus. Here then is a strong indication, that the Parian marble has adopted one of the computations of Ephorus, and it may also have possibly copied from Ephorus all the rest, as indeed some other circumstances give room to suspect; so that it has no claim to be considered as being an original system compiled by the inscriber himself, as many suppose, though without foundation. The chronology of Ephorus happens to be less known than that of any other Greek historian, although he was apparently the father of all the reductions made by his successors, only a little varied. That the system in the marble is not an original one appears also by the omission of the return of the Heraclidæ and all the Spartan kings, without which no new system could be well formed.



1000 years from the expedition against Troy down to the passage of Alexander into Asia.\* Alexander's passage was in 3d of Ol. 111, so that on deducting from 1000 the 443 years above, there remain 557 years from Troy to 1st Olympiad: when therefore those of the middle age reduced this sum to 508 or 520 they cut off only about 40 years; which sum they either cut off from the first interval of 120 thereby reducing it to 80, as we find it accordingly stated by Thucydides as early as in the *middle* age; or else instead of the 100 of the first interval and the 80 of the second, 20 were cut off from each, and the two together reduced to 140.† It is however true, that the text of Clemens must be corrupt in regard to some sums  
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\* " Ut autem Duris, a Trojâ captâ ad Alexandri in Asiam adventum anni mille." *Strom.* 1. p. 403. *Potter's Ed.*

† " Sunt, qui colligunt a Troiâ captâ ad descensum Heraclidarum annos 120 vel 180." *Strom.* 1. As 180 seems to be monstrous for two generations, I presume that we ought to read 100 [vel] 80, especially as otherwise no notice at all would be taken of the computation of 80, which nevertheless had occurred a little before in his account of Eratosthenes. We shall be the less surprized at those larger sums, if we recollect that Paterculus says " Orestes vixit annis 90, regnavit 70." *Lib.* 1.

mentioned just before that quoted above, and the same in some subsequent ones; but we may the rather presume the sum of Duris to be correct, because it is not probable, that a merely accidental corruption should have produced the round sum of 1000 years between the two expeditions of the Greeks into Asia. Moreover, if we add together the longest sums, which are found to be assigned by prophane authors to each of the three intervals, out of which the total 557 are composed, we shall find, that they will amount either to not much less than 557, or else to more. Thus, the first interval we have already found to have been extended to 120 years, although containing only the two generations of Orestes and his son Tisamenus, who was expelled by the Heraclidæ; but who survived his expulsion several years, and was at last slain in battle according to Pausanias, therefore could have scarcely been a very old man when expelled. Again, the second interval down to the Ionic migration consisted also of the two generations of Melanthus and Codrus, to which together could scarcely be allowed less than 80, while 120 were allowed to the first interval; and which first even Newton did not reduce to less than 80; nor did Eratosthenes reduce the

the second interval to less than 60, as we may see in Clemens. But why the above modern as well as ancient author should make that difference of 20 years between the two intervals, each containing only two generations, does not appear; it proves however that in some few cases they did not judge 40 years to be too much for a generation; therefore the earliest Greeks may probably have allowed 80 years to the second as well as much more to the first.\* The third interval from the Ionic migration  
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\* At p. 73 I said, "that Petavius had collected from the ancients the amount of the two first intervals to be 130 years," and again at p. 116 "that no author had made them to be more." These expressions may be too brief to be sufficiently explicit. I may therefore add to them now, that the *earliest* Greeks might possibly have made twice 80, i. e. 160 of the two together or even more, which however no ancient has expressly affirmed: but this we know, that in the age of sober calculation they were confined by Eratosthenes to 140 years; and that this sum in the *later* age of Strabo and Pausanias was reduced to 120, the former at 35 years to a generation having been thought too great an allowance. So that the 130 of Petavius is a medium between the two sums, and as great a sum as the later and better judgment of the ancients esteemed reasonable, if not greater. The sum therefore of Larcher 154 is very exceptionable, as it exceeds the largest sum, which is *known* to have been allowed, although we may not improbably *sup-*  
*pose,*



to Ol. 1st contains the reigns of 11 archons for life, which at 33 years each the usual allowance by Herodotus in the middle age, would amount to 363, and 2 more at least, if not 11 of the 12th archon Æschylus, make up 365 [all which Newton has reduced to only about 28 years]; now 365, if added to the 200 (i. e. 120 and 80) of the two preceding intervals amount to 565, that is, to more than the sum of Duris: which indicates that there is apparently no material corruption in his sum 557. Moreover, he is mentioned by Cicero as *homo in historiâ diligens*, in case that computation was his own; which it more probably was not, for as he was an historian of the Macedonian kingdom only, the origin of this did not compel him to go back so far into antiquity as the Trojan war; therefore he might

pose, that the sum of the *earliest* Greeks *might* indeed have been larger. Beside this, it is quite arbitrary and unsupported by evidence in another respect; for the sum of Larcher must have been exactly 154 in order to suit with his reasoning, and neither more nor less. Now this is to assume an accuracy in the computation of Greek antiquities, to which the ancients never pretended nor indeed could possibly arrive in their mode of reckoning by an average in generations: so that any conclusion derived from the premises of Larcher concerning the date of the capture of Troy must be too precarious to deserve attention.

might have been satisfied with copying some current date of that event, which had obtained in his own age; and this might have been that of the *earliest* Greeks, since he lived before Timæus though later than Ephorus; whose bold and novel reduction of a whole century in that date might not in the age of Duris have gained that universal approbation which it did afterwards. But whenever that reduction was made, it must have been done with the least inconvenience by shortening either the number or duration of those 11 archons, for scarcely any thing more seems to have been known concerning them except their names. Accordingly Syncellus informs us, that Africanus made some of those reigns to be longer, than Eusebius or himself did afterwards to the amount of 48 years, and possibly the *earliest* Greeks may have made those reigns still longer; but the longer sums of Africanus prove, that at least another considerable part of the reduced century was cut off at different times from this third interval as well as from the two preceding ones: yet even in the *later* age of Africanus it still amounted to 341, therefore all the three intervals together in the *earliest* age to more than 541, which is not so much less than the 557 of Duris, as to  
 impeach

impeach this earlier one of any corruption in the text of Clemens, but rather to confirm it. So that 40 or 50 years only of the *earliest* calculations were cut off by the Greeks in the *middle* age, and a whole century afterwards in *later* times, 563 or 557 being first reduced to 520 or 508, and at last to 407. When therefore we thus find, that nearly 160 years had been taken away from the Greek antiquities, we shall be more ready to admit that a similar reduction of 160 might have been made in the Assyrian antiquities during the very same age and for the very same purposes by the same Greek chronologers. This is also the more probable, because we have found, that it was during the middle age, in which Ctesias lived, that the Assyrian antiquities were first connected with those of Greece, and they were then fixed in harmony together; but were afterwards plainly disturbed and mutilated in the former nearly to the same amount as in the mutilations and reductions, which took place in the chronology of Greece itself. Now after this view of the manner, in which the capture of Troy has been varied in different ages in company with the Assyrian period, it is no less extravagant in Larcher to attempt to reconcile



all such discordant dates to any one sum and especially to so great a one as 494, than it has been in Newton to reduce it to the opposite extreme of 128; and this chiefly by shortening the above 11 Archons to an average of 20 years apiece instead of 35, although in the very same computation he acquiesced in the allowance of 40 years to each of the two generations in the first interval: and it must be equally extravagant in any other writer to contend for any one particular year as the true one; for the above variations prove, it can be only by mere accident, if any one of the above-mentioned different dates shall approximate within 50 years of the true time. But there is more credit to be given to the established computation by Eratosthenes than any other, because much historic evidence, which he had an opportunity of examining, has now perished; such as the catalogue of the priestesses of Juno at Argos accompanied with the duration of each, together with many traditions concerning the Spartan kings. Moreover the same evidence was afterwards examined again by Dionysius, and the decisions of Eratosthenes confirmed by him, who also wrote a tract on purpose to ascertain them, it being a subject materially

rially connected with his Roman history.\* The most apparent objection, which can be made at present to his date of the Trojan war, arises from what I have noticed already, concerning his allowance of 40 years to each of the two generations of Orestes and Tisamenus in the *first* interval; yet Newton himself has allowed the same, notwithstanding that he rejected any greater average duration of the Spartan kings than 20 years each (*p.* 56). In the *second* interval however down to the Ionic migration Eratosthenes has compensated for the former being too long, by making the latter possibly too short at 60 years only for these two other generations. There is also some room to doubt, whether there were not actually three generations in this interval, and if so, 140 years would not be too many for the five generations together.† In the  
*third*

\* “Esse incorruptas regulas, quibus Eratosthenes [in chronologiâ] usus est, alio tractatu declaravimus.” *Antiq. Rom. lib.* 1.

† This doubt is founded on the apparently youthful age, at which Melanthus seems to have been ejected along with Tisamenus from Lacedæmon by the Heraclidæ; whereby a long interval must have intervened before was chosen king at Athens. Some ancients indeed carelessly say, that he was already king at Lacedæmon before his expulsion, or at Pylos; but Pausanias shews, that neither was

*third* interval Newton has begun his catalogue of Spartan kings with Procles and Eurysthenes, and has omitted their father Aristodemus, who was slain immediately on the return of the Heraclidæ, leaving them infants; whereby they must have had a long minority, and their reigns equal to their whole lives, which ought therefore to be estimated at two generations; or else what comes to the same thing the generation

true; and also that he must then have been very young, by his being one or two generations farther distant from Nestor than the rest of his descendants expelled at the same time. He retired to Athens, and how long he lived there before he was made king is not known; but it might have been during the first half of his life, which would be equal to a generation: for a generation means the last half only of a man's life, whereby he commonly outlives his father, the other half commonly passes before his father's death. Now in the present case Pausanias shews, that Melanthus was the fifth generation inclusively from Periclymenus Nestor's brother, whereas the descendants from Nestor himself expelled at the same time were two of them in the fourth generation and one in the third only. So also Tisamenus was only in the third generation from Agamemnon cotemporary with Nestor. Melanthus then must have been a child. This is confirmed by the fact, that Xanthus king of Thebes, afterwards slain by Melanthus, was the fifth generation from Peneleus, who commanded the Bæotians in the Trojan war. (*Pausan. lib. 2. & 9.*) This shews, that Melanthus was not coeval with a third generation through premature deaths, but through his extreme youth only.



tion of Aristodemus should be included as the first Spartan king. (*Chron. p. 51 & 56. Herodot. lib. 6.*) Thus Louis XIV by coming an infant to the crown, had a reign of 70 years, nearly equal to his whole life: so that there were 18 kings in all and not 17 only down to the passage of Xerxes in Ol. 74. 4. who reigned 624 years; which is not 35 years each: this average cannot be objected to as exceptionable, since even in modern times in one succession, namely, in Bavaria 10 princes, all sons succeeding to fathers, reigned 351 years, which is full 35 apiece. What has therefore certainly happened in one instance in modern times, might have equally happened in one instance among the ancients, more especially as the Spartan kings always married late and lived temperate as well as active lives in a healthy climate. One other similar case occurs in antiquity, which also confirms the above fact, although no other case except that of Bavaria occurs in modern times: for in truth some successions must in course have exceeded 33 apiece, as well as other successions fall short of that sum, otherwise the ancients could not have conceived 33 to be a *general average*.\* The case of the

\* This other period alluded to was the succession of kings in Media after Dejoces, both the commencement

Roman kings has some different circumstances in it, which render it still less exceptionable.

mencement and end of which have been ascertained by the ancients with certainty; and it contained 150 years under only four kings, which amount to above 37 each: they were all sons succeeding to fathers, therefore equal to generations, and yet one of them was slain after only 24 years of reign, and another did not reign to his natural death, but was deposed by Cyrus. Newton therefore has certainly made too much difference between generations and reigns, in allowing to the former 40, 35, 30 and 23 years on an average, yet only 18 or 20 to the latter. This error forms the whole foundation of his system, and has been so sufficiently proved against him by his opponents, that Mr. Falconer has augmented the average to 24 in his late chronology at Oxford, which if not too little for an *universal* average, is still too small for *many* successions. Accordingly Sir James Stuart in a tract published by him at Francfort 1757 intitled *Apologie du Chronologie de Newton* has re-examined the succession in 10 modern states, and after some corrections allows that only 1 of those 10 forms so small an average as 20, the rest amount to 24, 26, 28, and one in Bavaria to 35 years for each reign during 351 years (*p.* 33) and this although he is a professed disciple of Newton; yet upon the following principle only, that all the 10 averages together form an *universal* average of only 24 years, notwithstanding that only one is so little as 20: but then as one average amounts to 35, why might not the same fact have happened once or twice in ancient times, which he thus allows to have happened once in modern ages? Such an *universal* average is only an useless curiosity if it be scarcely applicable to one *particular* average out of ten contained in history.

ceptionable. Thus we find no good foundation for rejecting the date of the Trojan war by Eratosthenes, although some ancients afterwards did reduce it by almost 50 years : but whether right or not, yet the above 405 was plainly the date to which the reducers of the Assyrian period accommodated the latter and which consequently contains an indication, that these variations and corruptions of it were made later than the age of Eratosthenes; so that the longest sums must have both been the original ones, and also been *well known* in Greece 200 years before Diodorus.

With respect to Eratosthenes himself there may however at first appear to be some doubt, what was his own original and genuine sum of the interval from Troy to the Olympiads ; for some of the ancients mention 408 and others only 407 viz. Censorinus, Clemens, Solinus, Eusebius, Diodorus, &c. but then the want of precision in their expressions affords no sufficient room to conclude but that they all meant the very same date, and only differed in the mode of expression, by sometimes reckoning exclusively of the year in which the capture took place, whereby they made only 407 ; while at other times by reckoning inclusively of that year they made

408 :



408 : so that if this was the only real difference between them, the year of the capture according to Eratosthenes was the 408th before 1st Olympiad; and this Petavius has accordingly attempted to prove the true year, not only as stated by Eratosthenes, but also as being the actual year of the capture, by an evidence, the validity of which we will examine. But as to 405 we have already found that this must have been the sum adopted by those authors who after the time of Eratosthenes reduced the Assyrian period to a conformity with the above reduced Greek interval 408, by its being exactly the sum left, after omitting the four reigns of 162 years : and to this sum Eusebius also bears testimony ; for although he has mentioned in one place 400 *alias* 406, yet in another he says 406 *alias* 405 [ap. Troiam. capt.] and in fact he has made up so many as 406 only by his variation in reckoning 315 after the omitted 4 reigns down to the end of the kingdom instead of the 314 in Berosus. So that the 408 of Eratosthenes and the 405 of his successors seem to form the only real difference among these authors in regard to that reduced interval ; for as to 400 it is mentioned no where else, and might possibly

sibly be nothing more than merely a vague computation by the nearest round sum. Now the evidence employed by Petavius to prove 408 to be the true year of the capture is copied from Dionysius, who says "that Troy was taken on the 23d of Thargelion 17 days before the summer Solstice, so that there still remained after it 20 days before the end of that Athenian year," which ended with the subsequent month Scirrophorion (*lib.* 1): accordingly Petavius shews, that this was the real fact in 408, but not in 407. For he calculates, "that in 408 the new moon, at which Thargelion began, fell on the 21st of May, from which day to the Solstice would intervene (he says) those 23 and 17 days, and thus the Solstice is placed on June 28:" but 23 and 17 amount to 40, whereas there were only 37 from May 21 to June 28 both exclusively; therefore Dionysius must have meant to fix the Solstice on the 29th or 30th, (for inclusively there would be still only 39) as it actually was in the age of Troy. This is confirmed by what is added, that there were 20 days after the Solstice to the end of the Athenian year: for the last Athenian month Scirrophorion began on June 20 exclusively, and ended on July 20, whereby it lasted 20 days after

the Solstice on June 29 or 30; thus the 23d of Thargelion would be the 13th of June not the 12th for the day of the capture. But although Petavius has not fixed the *Solstice* and the *day* right, yet he has the *year* 408; and rightly shewn, that the above circumstances would not correspond with the year 407, in which the new moons fell 11 days sooner. Thus far then this evidence is solid, and proves 408 to have been the real year meant by Eratosthenes, not 407; so that those ancients, who mention the latter date, must have reckoned exclusively of the year of the capture: but still this does not prove any thing in favour of 408 being the actual year of that event, agreeably to the title of that chapter *de vera Troiæ epocha* (lib. 9. 29.) For the 23d of Thargelion would have fallen on June 13 and only 17 days before the Solstice in several other years, at least once in every Metonic period of 19 years, and in two other years of it nearly the same. It would for example have suited nearly as well to the year 435, which is only one year sooner than the date in the Parian marble, excepting that the new moons would indeed have fallen two days sooner; but this would produce no other consequence, than that the Solstice would  
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be placed 19 days later instead of 17 only. Yet in all this erroneous account Petavius is blindly followed by Jackson vol. iii. 328, which points out the great benefit to be derived from my Calippic Tables, whereby such errors of Jackson and others may be detected without abstruse computations.\* Moreover, Scaliger, Petavius and Jackson are quite mistaken again in conceiving, that this computation was made either by Eratosthenes or Diony-

P p 2

sus,

\* For instance, if we wish to prove how the new moons actually fell in the 408th year before 1st Ol. we must add those 408 [as by the rule in my Appendix to vol. iii.] to 776 the date of the first Olympiad before Christ; the sum is 1184, from which deducting 330 the date of the 1st Calippic year, the remainder is 854, which being divided by 76 leave 18 remainder, and these 18 deducted from 76 leave 58, to which one must be added as by rule; so that 59 expresses the Calippic year then current in 408: and on looking backward for May, we find May 19 for the day of the new moon of Thargelion; to which we must add 3 as by the rule, because 854 contains nearly three periods of 304; thus we find May 22 for the day of the visible new moon, therefore the true new moon being nearly one day sooner, it fell on May 21 or early on May 22, exactly as Petavius found it to fall by a scientific calculation founded on Lunar Tables. This proves, that my Calippic Tables may be depended on at the distance of nearly 3000 years with sufficient accuracy for almost all questions, which can occur in history.

sius, and in conformity to the Metonic or Calippic period or their own computations, for it was a very ancient calculation made long before either of them existed, and was only accommodated by them to the year 408: that circumstance indeed of there being 17 days only before the Solstice may have been added by one or other of them, as being the necessary result, which they themselves perceived to arise from the day of the month in question as they found it to fall in the year 408 which they had adopted; but as to the day itself the 23d of Thargelion, this was a received opinion among the Greeks before the Metonic period was discovered; and it contains no circumstance which could enable any one to prefer one year before any other, or whereby they could ascertain the right year. For Plutarch informs us, that the 23d of Thargelion was the reputed day adopted not only by Ephorus, but also by Damastes.\* Now Damastes lived before the  
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\* “ Die 24 Thargelionis Troia putatur capta, ut Ephorus, Damastes, Calisthenes et Phylarchus autores sunt.” *Plutarch in Camillo*. This makes the date one day later, which if right, the Solstice would be one day later, therefore not before the 30th of June, as it apparently was in the age of the Trojan war, and not on the 28th as Petavius supposes:

the Metonic period was introduced, which was not until the commencement of the Peloponnesian war; for he was a scholar of Hellanicus: Dionysius indeed adds, that he continued until the *time* of Thucydides [*ἡλικίας*] which Greek word is very ambiguous, as it sometimes means *maturity* and sometimes advanced *age*; but even if thus Damastes did not write during the currency of the Octennial period, yet at least the opinion concerning the 23d day plainly did not originate with Eratosthenes; and before Damastes the 12th of Thargelion had been adopted by Hellanicus.\* So that both of these dates were ancient ones, yet neither of them would be sufficient to ascertain the right year, not even if the circumstance of the 23d falling only 17 days before the Solstice was as ancient as the date on that day; nay, under the currency of the Octennial period even this would have been of no service, as near the beginning of each period of 152 years it placed

poses: but in the time of Meto it had got back to the 27th, and in the time of Eratosthenes it fell still earlier.

\* Qui paulo ante Peloponnesiaca tempora vixerunt et usque ad Thucydidis *ætatem* processerunt sunt Hellanicus et Damastes et alii." *Dion. de Thucyd. Judic.* " Hellanicus dicit duodecimo Thargelionis." *Clemens Strom.* 1.



placed the Athenian months almost 30 days sooner than near the end of those 152 years. This evidence then from Dionysius is only so far useful, as it ascertains, that the year 408 was the one meant by Eratosthenes not 407, and it contributes nothing toward the determination of the right year: it afforded therefore no foundation to prevent later chronologers than Eratosthenes from preferring 405 to 408; for 405 would have corresponded equally well in all the other circumstances, except that the 23d of Thargelion would fall then 20 days before the Solstice; the new moons in 405 happening 3 or 4 days sooner than in 408, as may be seen in my Tables in the 62d year. But very little attention might be given to this alteration by those, who could intentionally omit 162 years of the Assyrian kingdom; and possibly they might judge it to be a visionary attempt altogether to ascertain the precise year; and quite sufficient in point of accuracy to come somewhat near to the dates required by that common opinion, which had been their motive for making the above omission, and which was, that it happened in the 28th reign of Teutamus.

Upon the whole then the sums 405 and 406, which we find mentioned only  
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by those authors after Eratosthenes, who adopted the reduced Assyrian period of 1300, help to confirm, that they arrived at these sums instead of the 408 of that author, merely because 405 or 406 were the exact intervals which remained after omitting the four reigns of 162 immediately subsequent to the 28th reign of Teutæus or Teutamius, under whom the Greek historians of the middle age had placed the capture of Troy, inclusive of the interval of 91 from the end of the Assyrian kingdom to the Olympiads. Consequently the nature even of these deviations from the 408 of Eratosthenes contain an *additional* indication, that the reduction of the Greek interval after Troy to that sum was the cause of a similar reduction being made by his successors of the duration of the Assyrian kingdom after Troy, from what it had been stated by the *astronomic* chronology current in Asia, and at the same time an additional evidence for the *antiquity* of that period of 1460.

To the other proofs however, which I have already adduced, that the Asiatic chronology according to the astronomic period of 1460 has nothing really historic in it, but is merely an arbitrary period assumed through vanity and astrologic imposition,

position ; I may now add, that the same Syncellus, who has made this period known to us, has at the same time shewn us how very much addicted the Chaldæans were to such astronomic periods in all other portions of their history. For he relates, that they had included the whole interval from the creation to the deluge within the bounds of another vast astronomic or arbitrary period of 432,000 years, composed of 120 lesser astronomic periods of 3600 years ; and this under 10 kings only at Babylon, some of whom reigned not less than 18 periods of 3600, but all of them exactly some certain *whole* periods of 3600 and neither more nor less.\* Is not this information of itself alone quite sufficient to satisfy us concerning the fabulous nature of all such Chaldæan chronology ? and can we reasonably expect that those, who begin thus, should end better ? Especially when we find, that even after the deluge they proceed in the same way, to compute the subsequent unknown times in like manner by some certain whole periods of 3600 and 120 years ; out of which they compose several still different vast periods of similar extravagance, until they come near to the Assyrian

\* P. 30 and 78 and 18.



Assyrian period in question of 1460 beginning with Belus. May not the manners of a man be known by his company? More especially when we find those manners to have been imitated by all his neighbours: for as I have shewn before, the Egyptians on the west had founded a similar astronomic period of 36,525 years upon that very Assyrian period of 1460; and in like manner we find that the Persians on the east made use of a year of 365 days only, which would cause a complete revolution of their new year in the same 1460 years. Nay, the very same Chaldæan period of 432,000 has been extended even to the Hindoos and Chinese at the extremity of the world; therefore must have been communicated by imitation. The Hindoos have down to this very day no other chronology than what is founded on this period of 432,000 formerly in use at Babylon, which they multiply likewise in an arbitrary manner: the same method of chronology and the very same sums are found likewise in Ceylon; and even the Chinese include the three first ages of their ancient history within the same Chaldæan period of 432,000 years.\* These mimics

VOL. III.

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\* De Maillas's Hist. of China, tom. 1.

point out sufficiently the manners of their archetype. A writer indeed in the *Asiatic Researches*, vol. 7. p. 401. asserts, "that this period is in exact agreement with the system of Newton." It were to be wished, that he had informed us of the nature of this pretended agreement, and what combination or multiple of any of the celestial revolutions will produce it, which neither Syncellus, Bayer, Jones, Gentill, Sonnerat or Davis have been able to perform; neither does it appear by any travellers into Hindostan, that even the Hindoos themselves pretend to know the original foundation of their favourite period; but which seems to me to be indebted for its origin to Chaldæan superstition connected indeed with astronomy: for if the Chaldæans conceived that the revolution of the Equinoxes was made in 36,000 years, when they assigned to each of the 12 signs of the Zodiac a tutelar deity, they seem to have honoured each deity with presiding over one whole revolution, and thus produced 432,000 by multiplying 36,000 by 12.\* But at least Mr. Baillie says "that as this period has no relation to any thing in nature either celestial or terrestrial, which by fixing the ideas

\* Diodor. lib. 2.

ideas of different nations, might have occurred to different persons, it is extraordinary enough, that this very sum should have been received as a mode of computation in so many different and distant regions." *Astron. Indienne Disc. Prelim. p.*

111. He might have equally wondered how Chaldæan astrology could have circulated so widely and so long; but the same weakness in superstitious opinions, which disposed mankind to receive the one, made them equally receive the other from such professed adepts in celestial and astrologic knowledge as the Chaldæans; whose information to others on such subjects was closely connected together as parts of one whole system of scientific imposition.\*

That the period of 1461 had circulated as widely in ancient times as that longer one of 432,000, and had held as distinguished a place in astrology with respect to its being a boundary relative to the rise and fall of  
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\* "As there were philosophers among the ancients, who allowed the universe to have had a beginning, but were not able to determine when, hence the Egyptian historians adopted such astronomic periods, as best suited their views, and accommodated their history to them, always preferring the longest periods, as those flattered most their vanity in being thought very ancient." *Chronol. Egyptien par d'Origny, tom. 2. p. 7. Paris, 1765.*



kingdoms and even of the existence of the world, is confirmed by a passage in Censorinus quoted out of some lost work of Aristotle; from which it appears likewise, that the error concerning it among astrologers preserved by Firmicus and pointed out in my note to p. 57, was no modern one invented in the later age of Firmicus under Constantine, but as ancient as Aristotle, therefore apparently universal among oriental nations.\* We need not be surprized, that the duration of the celebrated Assyrian kingdom should be limited by this same *magnus annus*, which we thus find to have been made a reputed astrologic boundary to the existence of the universe; and also to have been endued with the same privilege of extending from a creation to a deluge, as that longer period of 432,000 both among the Chaldeans and Hindoos, or as that period of 36,525 among the

\* “ Hic annus 1461 a quibusdam dicitur *Dei annus*: est præterea annus, quem Aristoteles *maximum* potius quam magnum appellat; quem solis, lunæ vagarumque quinque stellarum orbes conficiunt, cum ad idem signum, ubi quondam simul fuerunt, una referuntur; cujus anni hyemis summa est *κατακλυσμος*, quam nostri diluvionem vocant, æstas autem *εκπυρωσις*, quod est mundi incendium, nam his alternis temporibus mundus tum exignescere, tum exaquescere videtur.” *Cap. 18.*

the Egyptians produced merely by a multiplication of 1461 by 25.

The astrology of the Arabians was doubtless derived from the Chaldæans and Egyptians, as indeed the contents of some Arabic books on astrology given us by Hottinger sufficiently testify; in which we again find the cares of astrologers applied to portend the fates and durations of kingdoms as well as of individuals. One of those enumerated by Hottinger is, "*Astrologia judiciaria secundum veterum traditiones per Magiouri*. Another *Syntagma de dijudicandis rerum omnium quæstionibus, quæ astrologis proponi solent, per J. Casranio: multa profert ex Babyloniorum Persarum, Indorum, Ægyptiorum traditionibus*; compiled out of ancient as well as modern authors; among the former are *Aristotle, Arcana Hermetis, Pharchasi Babylonii, &c.*; among the latter several Arabic authors, *de usu astrorum in fabricâ talismarum e Babyloniæ populis—de arcanis astrorum circa Dynastias, regna, Angelos, bella, &c.*—*de calamitatibus et fortunâ—de generatione et nativitate Chalifarum, regum—de fato regum et prophetarum, &c.*"\*

\* In the edition likewise of J. Firmicus at Basil 1551 are included translations of some Arabic astrologers,

trologers, one of whom is *Messahalach*, whose 9th chapter is intitled *De Conjunctione Planetarum Superiorum*, which as he there shews, portends the revolutions and dissolution of *kingdoms*. "Scito quod res maximæ et mirandæ accidant ex conjunctione planetarum superiorum; cumque conjuncti fuerint *ipsi tres* in uno facie et aspexerit eos *Sol*, significabunt destructionem sectarum et regnorum et eorum mutationem et res maximas: hæc est conjunctio eorum *maxima*, quæ significat prophetiam et destructionem quorundam climatum et res maximas et multitudinem commotionis bellorum."

P. 117. Now such was the very conjunction in question at the beginning of the period of 1461 in the 120th year before Nabonasar, for then the sun and moon were in conjunction on the reputed day of the popular equinox and in opposition to the earth; but according to the above doctrine of Aristotle *all* the planets were in conjunction: it can be no wonder therefore that 50 years afterwards, when the Assyrian kingdom was destroyed, if the astrologers of that age conceived this important event to have been portended before by the preceding great conjunction of those celestial bodies. In subsequent times the historic event was confounded with that grand astronomic conjunction, which portended it; and from vanity or ignorance concerning the true time of the commencement of that kingdom, its origin was moreover extended backward to a similar grand conjunction of those luminaries 1460 intercalated years before.


Vid. Hoffinger's *Bibliotheca Orientalis*, pars 2d, p. 254.







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Place this Table at beginning of the Remarks on the Kingdom of Media. —  The Edge on the Right Hand to be bound in.

Place this Table at beginning of the Remarks on the Kingdom of Media. — The Edge on the Right Hand to be bound in



CRITICAL  
OBSERVATIONS  
ON  
BOOKS,  
ANTIENT and MODERN.  
NUMBER XIII.

[To be continued occasionally.]

CONTAINING

The true State of the History and Chronology of the Empire of Medes from the Dissolution of the Assyrian Empire down to the Persian Kingdom founded by Cyrus amounting to 256 Years; and proving from the cotemporary Existence, coincidence of Dates, Reigns, and other Circumstances, that the six Assyrian Kings mentioned in Scripture were the very same Persons with the first six Kings of the Medes enumerated by Ctesias, although under different Names, as given to them by the Persians on the East of Babylon, from those ascribed to them by the Syrians and Jews on the West of that Country, agreeably to a Hint given by Prideaux—as appears from the harmonious Testimonies of Polyhistor, Herodotus, Æra of Nabonassar, Eusebius, and Scripture, when compared with the Account of Ctesias, as abridged by Diodorus.

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M DCCC VII.



*Addition to the Note at p. 376.*

"They shall afflict them 400 years—but in the 4th generation they shall come hither again." *Τετρατὴν δι' ἔνια*, &c. Gen. 15. 16. Plutarch also confirms these different durations given to a *ἔνια*, and moreover explains the reason of the difference, for he says "that according to Heraclitus, when it means the space of time before men arrive at maturity, it contains 30 years, but when the utmost extent of any aged life this may be 108 years, for 54 would be but one half of such a life." *De defect. orac.* *ἔνια* is used in the 1st ch. of St. Matth., nearly in the same sense, except with this difference, that it does not imply any precise sum of years, but the *whole life* of each person there named, whether long or short. Herodotus elsewhere uses *ἔνια* in this sense for a *life*, *reign*, or *succession* without implying any determinate number of years. Thus in lib. 2. he says, that out of 330 *kings* or *successions* in Egypt [*ἔνιας*] 18 were Ethiopians. So that in this sense by *in the 2d generation* he might mean *more* than 80 years, without determining any particular sum, whether less or more than 100.

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ERRATUM.

P. 392 in note, line 2. For *than of the Macedonians*, read *than that of the Macedonians was*—lest those words should be referred to Polyhistor, who was dead before Cleopatra.

Vol. III.

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*intermixing*

*intermixing any modern Conjectures. So that now for the first time after the course of 3 Centuries, Readers will be able to know what the historic Evidence is, which has been preserved from the Antients concerning this period of Time; and what the Additions to it are, which have been invented by modern Chronologers, and which have perplexed the Jewish Scriptures.*

**W**E have now in the course of our *researches* concerning the *principles*, on which the Chronology of profane History has been founded, arrived at that portion of it, which first gave rise to these enquiries, namely, the interval which intervened between the end of the Assyrian kingdom destroyed by the Medes under Arbaces, and the end of that Median empire which succeeded under the posterity or successors of Arbaces, until it was destroyed by Cyrus in the 55th Olympiad. This is to Christians one of the most important periods of ancient history, because the whole of it was cotemporary with the most interesting transactions of the Jewish kings, from the earliest mention made in Scripture of any king of Assyria, down to the return of the Jews from captivity, on the accession of Cyrus to Babylon. Now the



the chief difficulty which occurs during this interval is, to determine who those kings were, that are called in Scripture kings of Assyria ; for Scripture makes mention only of an Assyrian succession of kings, while profane history speaks only of kings of the Medes as reigning in Asia during the same interval : but unfortunately it has so happened, that this important period is the least known of any portion of profane history, so that it affords very scanty means to remove that difficulty and to clear up all doubts concerning it. One cause of this deficiency here in profane information may indeed be, from its being so very early a period in the history of the world as to hold a middle place between the fabulous, or at least unknown events of the celebrated Assyrian kingdom, and the first dawning of authentic history at the accession of Cyrus : but another and possibly the chief cause of that deficiency is from the accidental loss of all the antient professed histories of those times, such as the History of Assyria by Herodotus, that of Asia by Ctesias, the Universal History of Nicolaus Damascenus in Greek, that of Trogus in Latin, except a brief abridgement of such part of it by Justin as relates to Macedonia chiefly ; but more particu-

larly by the loss of the 5 books of Diodorus between his 5th and 11th, in some of which his own statement of the history of the interval in question must have been contained, and this probably in many respects different, or at least more explicit than in his brief abridgement of the history of these times by Ctesias, in his 2d book. So that very little has been preserved relative to this subject, except the above-mentioned abridgement of Ctesias; and in this Diodorus has sometimes expressed the accounts of Ctesias so very briefly, as to render his meaning ambiguous and obscure, intending doubtless, that his readers should have recourse for farther information to his own statements in such of his books as have perished. Unfortunately again, Photius, who has likewise made long extracts from Ctesias; has entirely omitted the contents of the first 6 books, which alone related to Assyrian history: and Herodotus also purposely omitted the same in his history still extant, because they made part of his other history of Assyria; so that he notices only some of the transactions of the 4 last kings of Media after the usurpation there by Dejoces, and even these but briefly down to Cyrus. In this distress however, the moderns have endeavoured to supply

supply the deficiency of original history, by connecting with the brief accounts by Ctesias and those in the Jewish Scriptures, some few other disjointed scraps of Median and Assyrian history, found incidentally mentioned in some other antient authors, who have escaped the general destruction : but in effecting this they have given themselves up so much to their own arbitrary conjectures, that no two of them agree in the same opinions ; and all of them disagree in some points or other with those few historic relics of the times in question, preserved either in Scripture or profane authors. The *Chronology* likewise of this period they have rendered as variable and uncertain as its *history*, of which this fact is a sufficient proof, that different modern writers have fixed the commencement of the Median empire by Arbaces at a variety of different dates, during a long range of time of no less extent than 200 years ; and yet they all pretend, that their accounts agree with Scripture. The confusion and uncertainty arising from these incoherent and conjectural historic systems by the moderns is augmented still more, when we seek for information from the antient Christians Eusebius, Sulpitius, Jerom or Syncellus, who seem to have given almost as little attention



attention to profane history as the Jews themselves; for they continually confound together the kingdoms of Assyria, Babylon and Media, without any due notice of the revolutions, to which those kingdoms had been subject in different ages; whereby sometimes one was at the head of government and sometimes another, sometimes also they were united under the same king, and sometimes formed three distinct kingdoms. So that instead of our deriving better information from such antient authorities, they rather perplex us the more; and we have as often to correct their errors of negligence as the contradictory and ill-founded conjectures of the moderns. We need however the less to wonder at the negligence of the antient Christians concerning Assyrian history, when we perceive, that they were equally negligent concerning Jewish history as the Jews themselves; for not any one of them, who have come down to us, seems to have had any right conception of the events at which the 70 years of captivity predicted by Jeremiah either began or ended. Africanus, the eldest of them, differed in this point from Eusebius and others in later times, and yet not any one of them was right in their account: in this they only followed  
again

again the negligent example of the Jews themselves, for not only Josephus, but the oldest Jewish chronicle now extant, Seder-olam, and the best modern Jewish historian Ganz, were all equally mistaken in this article [*v. vol. i. p. 136, and vol. ii. p. 419*]. The modern Christians however, have now come to an agreement in this event, but the other subject of the Assyrian, Babylonian and Median kings, is still involved in greater perplexity than ever; which has not only afforded Bolingbroke and Collins too much foundation to accuse the history of these times, profane as well as scriptural, of the utmost uncertainty, but has likewise excited such difficulties as to involve the most candid and inquisitive readers in a wilderness, whether they search the Scriptures with any commentators in their hands or without them.\* It must then be a  
 matter

\* See one example of this in no less able author than Lowth, B. of London, as quoted in my vol. ii. p. 340, where he supposes the Medes to have been in a state of anarchy and an inconsiderable people ever since the fall of the Assyrian kingdom; although in fact they were at the time there referred to at the head of Asia, and still rising in power, they having then established two kingdoms instead of one; who, though enemies to one another, yet were both of them a terror to all neighbouring nations, and one or other of them actually ruled over the chief part of Asia for a century afterwards.

matter of great importance to the veracity  
 of Scripture, as well as satisfaction to all  
 readers of it, to remove these doubts and  
 difficulties, which have hitherto obscured  
 this interval of nearly 300 years between  
 the Assyrian and Persian empires; during  
 which time not only Ctesias but all other  
 profane authors acknowledge the Medes  
 to have been the chief power in Asia [*v.*  
*vol. ii. p. 354*]: and notwithstanding the  
 scantiness of historic materials now extant  
 it appears to me, that there is still a suf-  
 ficiency preserved to remove the confusion,  
 which has been introduced into the subject;  
 in case we abstain from any system of ar-  
 bitrary conjecture, and confine ourselves  
 scrupulously to such historic evidence,  
 however slight it may be, as has been for-  
 tunately preserved from oblivion by Ctesias,  
 Herodotus, the æra of Nabonassar, Poly-  
 histor, Nicolaus Damascenus, Eusebius,  
 Syncellus and Justin; many passages of  
 which authors contain information, that has  
 either not been attended to at all, or not  
 rightly connected together. Readers in-  
 deed may naturally wish, that still more  
 full and explicit accounts of the history of  
 those Asiatic nations during this period had  
 been transmitted down to the present times;  
 but under the circumstance of the great  
 scantiness



scantiness of the materials which have been preserved, it becomes the more necessary to examine what does still exist in a very minute and critical manner: which therefore I have accordingly collected together out of the relics and ashes of former antient histories; and also so disposed the evidence, which results from it, into a table, that any one may almost at one view see the harmony contained in it with their eyes, as well as comprehend it by degrees from a concomitant illustration in words. There has however been one modern author, and only one, namely Prideaux, who has in some degree anticipated the true statement of the history of these times arising hence, with respect to the main substance of it, that is, the identity of the Assyrian kings mentioned in Scripture with the kings of the Medes enumerated by Ctesias, notwithstanding that their names are totally different; and yet his account has never hitherto been adopted by any other person either in Britain or foreign countries: this may at first give an unfavorable aspect to the fact, but the cause of this ill success has been from his omission to subjoin any proofs whatever in support of it; so that hitherto it has rested solely upon the credit of his own opinion, neither was he in all

articles right in his opinions. His omissions then I shall supply and correct his errors, while I build upon the foundation already laid by him, by comparing together the accounts of the times in question as given by seven different antient authors, partly profane and partly christian; some of whom ascertain the truth of some facts and some of others, but all of them contribute some assistance, and on the whole they meet together in as much harmony as can be reasonably expected from *unconnected* testimonies to the same facts and dates; which are always a stronger evidence to truth than the connected authority of any single author, as this may have been forced into harmony intentionally. If still any shadow of doubt should remain concerning the above-mentioned identity, yet I presume, that it will be only with those readers, who have had their minds already prepossessed by some prior and favorite modern author, or commentator on Scripture, to whom it may be therefore unpleasant to reject what they have long received as true. The evidence indeed to the contrary of their former opinions, may possibly sometimes appear to them but slight, yet still if it be antient evidence now existing, it must be preferable to opinions founded on no  
 historic

historic evidence whatever, and subject to all the immense variations and incoherence of modern conjectures. We can only make use of such evidence as we happen to have, but a new and stronger evidence will result from this, by the harmony hereby produced between profane and Scriptural testimonies as well as between different profane authors, who otherwise will be at variance; and harmony is always an essential effect of truth, consequently one of the means of trying, whether it be the truth or not. If this my new statement of historic facts and the harmony attending it have been hitherto hid from our eyes, it has been only in consequence of the numerous and ill-founded suppositions of the moderns; which have perplexed the subject, by substituting their own conjectures instead of antient evidence; so that both they themselves and their readers have overlooked and neglected articles of historic information transmitted to us; which really tend to connect together a true account of the history and revolutions, that took place in the three Asiatic nations in question, during the above-mentioned interval of 256 years.\*

S s 2

Now

\* I have hitherto delayed bringing forward my  
*Illustrations*



Now one of the first steps, which was necessary in order to arrive at a right knowledge of this period of history, was to ascertain the dates of its *commencement* and *end*, which in course gives us its total *duration*; and reciprocally the information preserved

*Illustrations* and *Corrections* of various articles in the preceding volumes, in order that I might the sooner arrive at the discussion of this important period of Chronology, and in hope of sufficient health and opportunity to complete it; so as to remove the former obstructions, which perplexed even candid readers of Scripture, together with the foundation of various sceptical objections, deduced chiefly from our ignorance of the history of the times in question, on account of the loss of all the professed antient historians, who had treated of the subject. In those *Illustrations* I shall include answers to some criticisms made on parts of the preceding volumes, particularly to one introduced into Mitford's History of Greece, which objects to my proof of the later age of Homer, than usually assigned to him, founded on the name *Miletus* being mentioned in his poems; which city, as Mr. Mitford contends, was rather founded and named much earlier than I have supposed. But Mr. Mitford has not considered, that the earlier account, which he gives of its foundation was only a vulgar tale mentioned by Pausanias as current in the city itself, and much of the same stamp as that of King Brute being the founder of London. Such popular tales are current in most countries, although without any credible foundation in truth: whereas the account given by myself was that of the most judicious Greek antiquaries; between which two Pausanias himself always makes a distinction, if he mentions both.

preserved concerning its duration gives some assistance towards the determination of its commencement and end. These objects therefore I made my first subjects of enquiry, and they have been already in some degree ascertained. For I began these *researches* by determining first of all the date of the accession of Cyrus, which put an *end* to the empire of Medes; and with respect to this event we discovered no greater error subsisting than one or two years: yet even this small variation from the true date we shall hereafter find to be of some importance in accounting for an apparent disagreement to this amount in the computations of some antient authors relative to that accession; which when thus corrected will be restored to harmony. But I have shewn moreover, that at the *commencement* of the empire of Medes, a much greater disagreement subsists among antient as well as modern authors, concerning the true date of this event; for it amounts to a whole century among the antients, and to not less than two centuries among the moderns.\* This rendered it necessary for me to enquire concerning the cause and origin of so great a disagreement, and

\* Vol. iii. p. 43, and afterwards.

and also to determine as to which was the true date : but this could not be done with any certainty without first of all examining the nature, dates and duration of the celebrated Assyrian empire, which *ended* where the kingdom of Medes *began*, at the accession of Arbaces after his dissolution of the preceding Assyrian kingdom. Now concerning this Assyrian duration we found, that there were two very different accounts transmitted to us by antient authors ; in one of which a very long duration had been assigned to it, which had originated among the Asiatic nations themselves, but which had the appearance of having been adapted by Asiatic astrologers in an arbitrary manner to the length of a favorite astronomic period, agreeably to a common practice in that age among oriental nations, of which relics are still subsisting in Hindostan : but that another and shorter duration has been transmitted to us by the Greek historians, to which more credit is due ; as it had apparently been derived by the inquisitive historian Herodotus from the Greek states, who had settled in Asia as early as during the existence of that very Assyrian kingdom and even 300 years before its *end*. These must therefore in all probability have received from their ancestors by tradition  
some



some knowledge of historic facts concerning it, at least concerning its *end*, if not also of its reputed *commencement* : since it had begun according to their traditions not above two centuries before those Greek colonies had settled themselves in Asia, and it ended not above 350 years before the existence of Herodotus himself; who was born in one of those Asiatic colonies, and must have collected his historic accounts from the traditions current there, or at least might have ascertained thereby the accounts of preceding Greek historians. Hence then probably was the origin of the two different durations of the Assyrian empire found mentioned in antient authors : one of them having been extended to a duration exceeding the truth by the Asiatic sages themselves, agreeably to the vanity and astrologic conceits of their pretended scientific knowledge concerning the influence of the celestial bodies on human affairs, both past and future; but the other not erring farther than traditional computations of time might naturally vary from the greater exactness of historic information. This latter therefore must come the nearest to the true dates of the *end* as well as of the *commencement* of the Assyrian kingdom; more especially since we find  
this

this traditional and *historic end* of it to be in harmony with the most accurate account of its *end*, which we are able to deduce from the information occasionally contained in the Jewish Scriptures relative to events mentioned there, which occurred in Asia, and are related by natives of that country; and at the same time in harmony likewise with the professed Chronology of the Christian authors Eusebius and Syncellus, who lived at the distance of 500 years from one another, and yet agree together in the same date for the *end* of the Assyrian kingdom and *commencement* of the Medes: and this a date moreover, no way connected by them with any Scriptural events, therefore it must apparently have been copied by both of them from some profane Chronology, either the history of Herodotus or that of Castor, Thallus, Apollodorus, Diodorus, or some other. It is observable still farther, that Syncellus cannot reasonably be suspected to have copied this *later* date of the *commencement* of the Medes from Eusebius only, any more than from any events in Scripture; because he severely blames Eusebius for having brought down to *too late* a date the commencement of the Assyrian kingdom, but makes no similar complaint against him for having  
also

also brought down the commencement of the Median kingdom *later* than any other authors; on the contrary, he adopts nearly the same *later* date; and yet there are other articles in which he differs greatly from Eusebius in the names, order of reigns and particular durations of them during this Median kingdom; which are sufficient proofs, that he was not a mere copiest of Eusebius in the date of its commencement, but had extracted the whole from some different profane Chronology now perished. Neither Eusebius indeed nor Syncellus have informed us to whom they were indebted from their Chronology here, but from its harmony with that of Herodotus we may not unreasonably presume, that it was either derived directly from his history of Assyria, or else from some later authors who had revived and adopted it; such as Castor, Apollodorus or Diodorus in one of his books, which have not been preserved, but must have contained his own accounts of these times. While on the other hand some other Greek historians who lived soon after Berosus, have been misled by his pretendedly better Asiatic authority to assign an *earlier* date to the *commencement* of the kingdom of Medes, namely that of 91 years before the 1st Olympiad in preference



to the 40 of Herodotus and his followers: this error again they probably copied from that arbitrary *astrologic* period of 1460 above-mentioned, which made the Assyrian kingdom not only to begin *much too soon*, but also to end *somewhat too soon*; for it made that kingdom to *end* along with that same astronomic period then current, and this was in the 120th year before Nabonassar, therefore about 50 years sooner than the actual *historic end* of that kingdom according to Herodotus, Eusebius, Syncellus, and the Jewish Scriptures. This circumstance then, that the Asiatic computation of the Assyrian kingdom made it to end *precisely* at the *end* of a period of 1460, in the 120th before Nabonassar, although 50 years sooner than the truth, as we are almost certain from all other historic information now extant, contains an additional proof to what has been adduced before, that the Asiatic *commencement* and *duration* assigned to that kingdom are as distant from the real truth, as the *end* of it is thus proved to be by historic evidence still preserved; and as would probably have been equally proveable in those other cases, if any historic evidence so antient had been preserved concerning them. *Ex pede Herculem!*

Having

Having by these investigations determined both the true *beginning* and the *end* of the empire of Medes, it now remains, that we proceed to fill in this historic interval of 256 years with the several chief events and the succession of reigns as well as revolutions, which took place in Asia during that time, between the accessions of Arbaces and of Cyrus: this enquiry will at the same time bring to our view farther evidence in confirmation of the true dates both of the beginning and end of that Median kingdom; and thus shew the preference due to the *historic* duration of 256 years before the 307 assigned by the astrologic period of 1460. Now the first important fact, which presents itself for our consideration during this interval, causes some difficulty at the *very* outset as before-mentioned, and has in consequence given rise to much altercation and many different suppositions among modern Chronologers; this is, to account for that apparent disagreement during this interval between the information, which Diodorus has abridged out of Ctesias, and what is afforded us by the Jewish Scriptures. For Ctesias relates, that Arbaces was succeeded by 9 or 10 subsequent kings of the Medes, being either his lineal posterity or at least succes-

sors to his throne and of the Median race; who reigned at first at Ecbatana, to which city Arbaces had according to Ctesias removed the seat of government, instead of Niniveh in Assyria: while on the contrary the Jewish Scriptures make mention during the same period of only kings of Assyria, *one* of them dwelling at Niniveh, without the least notice of any kings of the Medes whatever. Now in order to account for this seeming contradiction and prove it to be only an apparent not a real one, we shall meet with more obstacles from the many erroneous and indeed extravagant suppositions of the moderns, than from any material difficulty in the case itself, as stated by Ctesias on the one hand and by the Jewish Scriptures on the other; for it admits of a very simple and obvious solution: while on the contrary the moderns have adopted so great a variety of ill-founded plans for the solution of this difficulty, that I cannot undertake to state the nature of all of them, but will however afterwards reduce them under a few *general classes*. The true cause then of the apparent contradiction above-mentioned is only from the too great brevity of Diodorus in his abridgement of Ctesias, which sometimes renders his historic account ambiguous and  
obscure,



obscure, in course therefore imperfect. This account nevertheless clearly proves, that Arbaces after his conquest of the last king of Assyria Sardanapalus was acknowledged by all the allies, who assisted him, as king in place of that Assyrian king, *over all the dominions* formerly possessed by that monarch; consequently just as much king over Assyria as over Media, and even over Babylon likewise. This is expressly thus related by Diodorus: “ Qui a rege Assyrio defecerant, Arbacem regiâ indutum trabeâ *omnium rerum potestate uni permissa regem* appellarunt. Is cûm sociis præmia distribueret et *suos provinciis Satrapas* designaret, Babylonius quoque Belesis accessit, qui *imperium* ei predixerat, meritorum que *regem* admonens *Babyloniæ præfecturam* sibi *ab eo* tradi postulavit—Arbaces igitur Babylonem citra pensionem tributî obtinendam *ei concessit*—et apud *provincias* Arbacem *regno* dignum esse omnes censuerunt.” Nothing can be clearer here than that Arbaces succeeded to the whole power over all the provinces of the former Assyrian kings; and might therefore in strictness have had his titles enumerated as king of Assyria, Media and Babylonia, to which provinces he had thus appointed prefects to govern them under his own controul, as  
supreme

supreme king over all. But Arbaces being a Mede himself, and having effected this revolution chiefly by the assistance of the Medes, and having afterwards removed the seat of government from Niniveh in Assyria to Ecbatana in Media, it was natural enough for Ctesias or any other professed historian to speak of him afterwards as king over the Medes rather than over Assyria or Babylon; which hence became only dependant provinces, although he was in effect equally king over them all. But with respect to the Jews and other distant nations, who had no concern in the revolution, nor were any way affected by it, when they saw, that these different kingdoms still remained possessed by *one supreme king* as before; and that the only alteration caused by the revolution was that of substituting a new race or family on the throne, they might just as naturally have still retained the former title of *king of Assyria* only; more especially since Assyria lay nearer to Judæa, and was better known to the western nations than the more distant and hitherto more obscure province of Media. We have a similar case, which occurred in Asia immediately afterwards in regard to Cyrus; who being a native of Persia subject to the king of Media, when  
 he

he had fixed himself on the throne of Media, both those provinces remained subject afterwards to one monarch as before. Hence the Scriptures generally mention the titles of *Medes and Persians* as united; but Herodotus on the other hand calls Cyrus and his successors kings of Persia only and his army a Persian army, seldom mentioning the name of Medes: while Thucydides on the contrary calls his successors Darius and Xerxes by the name of Medes, and their army a Median army only, scarcely ever mentioning the name of Persians. Another example of the same kind occurs in the present times, for although a prince of a Tartar race now governs China, and the chief officers and army are made up of Tartars, yet European nations still give him the former title of Emperor of China, just as before this revolution, and just as the Jews did with respect to Assyria; for they still continued to assign to the reigning prince that antient and well-known title of Assyria, rather than the name of the new race from which the later reigning princes there were descended, namely that of Medes. All such variations as these depend merely upon many accidental circumstances of the times; which do not afford the least foundation,  
arising



arising solely from this variation of the *title*, that those monarchs, called kings of Assyria in Scripture were different *persons* from the kings of the Medes in Ctesias; since it thus appears that they ruled over both those kingdoms at the very *same time*, which is the only material article of consideration. Nay, it is possible, that Ctesias himself might have sometimes actually called these kings by the name of *Assyrian*, although Diodorus in his brief abridgement has omitted it; but nevertheless he has done what is equivalent, by having plainly said in words, that they were kings over *all the provinces* of the former kings of Assyria. It is therefore probable, that there never would have been the least doubt among the moderns of the identity of the kings of Assyria in Scripture with the kings of the Medes in Ctesias, if they had but been clearly convinced of their living in the *same times*: but it unfortunately happened, that it was not obvious from the computations of Ctesias or Scripture, whether this was true or not; especially since the Jews themselves and the antient Christians misled by them, had placed the accessions of the Jewish kings *too early*; so that they might possibly have been contemporary with some of the *last* kings of the

the preceding Assyrian succession, and not with the *first* kings of the subsequent Median succession of monarchs in Asia. In this error they have been followed by many modern chronologers; but when this error is removed, as it necessarily will be, whenever the accessions of the Jewish kings are fixed right, that is, agreeably to the dates of the cotemporary Babylonian kings in the æra of Nabonassar, as they accordingly are for the first time in my table at vol. ii. p. 216; and when also the true date of the *historic commencement* of the Median kings by Arbaces shall be rightly fixed, which is our subject now under consideration, then all other difficulties will be removed at the same time: for then it will appear, that the *earliest* Assyrian king mentioned in Scripture *Phul* could not possibly have reigned until *later* than Arbaces in Media, and that there is ample time intervening between *Phul* and the accession of Arbaces for the reign of *Jarib* mentioned by Hosea as king of Assyria; who must thus have been cotemporary with Jereboam 2d of Israel, therefore apparently the very same person as Arbaces, as may partly be seen in my table at vol. ii. and also better in my annexed table here. These right accessions of the Jewish kings have accordingly been

adopted by Mr. Falconer at Oxford, since the publication of my vol. ii. and now that they have at last obtained their right places in a *system of Chronology* it is to be hoped, that they may ever retain it hereafter;\* for this single correction, although it amounts only to a few years, will be found to produce

\* It must indeed be allowed, that Newton had long ago fixed the accession of Jehoiakim nearly right, i. e. [“ 6 months *after* the year of Nabon. 139,”] viz. in 140, and yet erroneously a few lines afterwards he places his 4th in 142 instead of 143, and Zedekiah in 150 instead of 152 [p. 297]; thus only 1 or 2 years too soon, which however if he had extended his dates backward would have fixed the preceding Jewish kings nearly right; but he destroyed the connexion of these kings with the right Assyrian ones by *supposing* Asarchaddon to be the Sardanapalus of Ctesias, which opinion Jackson borrowed from him to the detriment also of his own account. In the Jewish reigns however he rightly followed Newton; but he again spoiled all by the above *supposition* and others of his own added to it concerning the succession of Assyrian kings; and by thus intermixing truth and error alternately it did not appear, that the *Jarib* of Hosea must necessarily be cotemporary with the *Arbaces* of Eusebius, and probably be the very same person; which lays a foundation for the identity of the following 5 kings in Scripture with the subsequent 5 kings of the Medes in Ctesias, and this becomes confirmed by subsequent dates in the *æra* of Nabonassar. Hence we see the necessity of determining rightly the *historic* accession of Arbaces, that we may know in what year it falls of the reign of Jereboam 2, and if only 40 years before 1st Ol. or otherwise.



duce unexpected harmony with the dates of profane authors, both before and after the Jewish captivity : and this *harmony of dates*, considering the great deficiency of *historic* information, is the best testimony, to which we can have recourse instead of it ; and one by which we ought in all reason to abide rather than by the wildness and discordance of mere *conjectural* opinions, in which we shall find, that scarcely any two authors can agree together. Neither is this identity of the kings of the Medes in Ctesias with the Assyrian kings in Scripture, although first revived by Priedeaux in modern times, altogether a new opinion ; for it appears by Syncellus, in one sentence, that he himself or some others from whom he borrowed it, had conceived the same opinion long ago : he is however so unsteady in this as in many other articles, that he has not always adhered to it ; but at least he did not deserve the censure, which Marsham passes upon him. For Marsham says, “ Syncellus sibi non aliis satisfecit, quum *Medos hos reges Asiæ etiam Assyrios* vocari voluit.” (*Sæc.* 17.) Yet why has he not satisfied others ? In fact he says only the very same thing, which the present words existing in Diodorus and extracted from Ctesias had said long before,

and which I have already quoted, excepting that the single word *Assyria* is wanting instead of what is equivalent to it, *omnium provinciarum regem*, of which provinces *Assyria* was the principal.\* This account then of Ctesias confirms the information of Syncellus, and shews, that in his time there were profane histories of Asia existing, which had set this subject in a clearer and the true light; to which the antient Christians might have had recourse for better information on it than they have manifested: but they were too apt to imitate and follow the accounts they received from the Jews, who from principle neglected all profane history, and communicated nothing to the Christians except such corrupted traditions concerning profane events as they had received from their ancestors, and were current among themselves; of which we find too many examples even in Josephus, whenever he deserts profane

\* Μηδων μεν γένος κατά διαδοχὴν ἀπὸ Ἀρβακου ἐβασίλευσεν ἡδὲ βασιλεία καὶ Ἀσσυρίων ἐλέγετο διὰ τὸ εὐγενὲς καὶ παλαιὸν τῆς ἀρχῆς καὶ τὸ τῆς χώρας ὄνομα, καὶ Μηδὼν διὰ τὸ ὄψιμον τῆς ἐξουσίας καὶ τὴν χώραν Μηδείαν λεγόμενῃ. Medorum genus regnabat ob Arbace per successionem; ipsum vero regnum *et Assyriorum* appellatur ob nobilitatem et antiquitatem dominii et regionis nomen, et *etiam Medorum* ob nuperam potestatem ac regionem Mediam dictam. P. 205.

profane historians. Hence arose the ignorance of the antient Christians concerning the history of Asia, and their consequent confusion of the three kingdoms of Media, Assyria, and Babylonia, of which Syncellus himself has likewise given too many examples, even although he thus appears to have known better. But this is less excusable in Marsham, as being a professed historical critic, and yet not to have distinguished better between truth and error; and in fact it required no great sagacity, if his thoughts had not been shackled with prior errors derived from more antient authorities; for the matter amounts to neither more nor less than as thus. Æschines in his oration against Demosthenes had occasion to mention twice some acts done *by the Medes* [απο Μηδων] and afterwards he twice mentions other acts done by the *king of the Persians* [των Περσων ο βασιλευς] one of which happened in the very time of Demosthenes like one of those concerning *the Medes*: now would not a reader be considered as very deficient in history who should conceive *the Medes* here in question to be a different people, and subject to a different king from this *king of the Persians* referred to in the same short tract and living at the very same time? Yet here the same  
speaker



speaker in the same oration calls the subjects of the same kings by two different names to the very same Athenian audience, and no person ever yet found any difficulty in the matter. Why then should the Scriptures be deemed at variance with profane authors for giving the title of *Assyrian kings* to those, whom the Greeks called rather *Median kings*, since they were kings of both nations just as the other was king of both *Medes and Persians*; and the very same historian Diodorus has just as well informed us of the one fact as the other? One really stands amazed, that such a man as Marsham and so many others should have ever puzzled themselves so much about such a simple question; and this in the very same moment that they allow Diodorus to have mentioned *the confederates as having given to Arbaces dominion over all the possessions of the deposed king* [τὴν τῶν ὅλων ἐξουσίαν αὐτῷ ἐπέτρεψαν]. There never was then from the first the least ground for supposing the kingdom of Medes in Ctesias to be different from the kingdom of Assyrians in Scripture, except what might arise from our ignorance of those two kingdoms having existed at the *very same time*; as in the case of the kings of the Medes and Persians by their union under Cyrus, just as the

the other two kingdoms were united under Arbaces. It is therefore upon the *Chronology* of those times that the chief proof of such an identity in these kings depends, and this it is my object to prove in the annexed table, that the fact may be rendered as obvious and of as popular a nature as possible ; of which right *Chronology* there is sufficient proof still extant, although very little profane *history* has indeed been preserved concerning those kingdoms, so that it is fortunate that the question depends as much upon *Chronology* as *history*. For instance, the *historic* evidence concerning the *times* in which those Median kings must have lived, may be confirmed by shewing, that the 6th of those kings *Artæus* both *began* and *ended* his reign in the very same years as the 6th Assyrian king in Scripture *Esarchaddon* ; moreover that his two successors in Ctesias both began and ended their reigns in the very same years as the two successors of *Esarchaddon* in the æra of Nabonassar ; which three kings were also in both cases the very last kings over Assyria before that kingdom and its capital Niniveh were destroyed by Cyaxares ; so that this event likewise took place in both cases in the very same year. Hence those three Median

dian kings in Ctesias and the three last Assyrian kings in Scripture, before the end of the Assyrian kingdom, will be found to have reigned over the very same three nations of Assyria, Babylonia and Persia at one and the same time, which is impossible, if they were not the same persons: and they would also have all equally reigned over the fourth kingdom of Media, if it had not been separated from the other three kingdoms at the very commencement of Esarchaddon by the usurpation of Dejoces; as we are informed by profane historians, and it is confirmed also in the Jewish Scriptures. But if this was the real fact with respect to those three kings of Ctesias, that they were the same persons with the three last kings of Assyria in Scripture and the æra of Nabonassar; we may hence reasonably conclude that the case of identity was the same with the preceding five kings of the Medes in Ctesias, since the very same number of five Assyrian kings are found in Scripture before Esarchaddon: and it is proveable also from Scripture, that two of them Tiglah and Salmanassar ruled in Media likewise, for they planted there colonies of captive Israelites, as well as in Assyria. These facts and coincidences both in *History* and *Chronology* have never been



been hitherto pointed out by any writer before, and they add indubitable testimony to the information by Ctesias of the union of *all* those nations under *one king* Arbaces [uno rege], as quoted already from Diodorus; whereby they sufficiently supply the want of evidence in Prideaux, concerning the identity of the Scriptural and Ctesian kings of Assyria and Media: by these means then will in course be removed also the present chaos of contradictory and conjectural opinions, which have now during three centuries been offered by Chronologers and Commentators on Scripture; and which have only perplexed the subject still more and more, instead of reconciling Scripture with profane authors, or the latter with one another.\*

VOL. III.

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\* It may be useful, if I here collect together into one view the little which Prideaux has said on this subject, with some remarks upon his account. [*Ann. 747 bef. Christ, the 1st of Nabon.*] The antient kingdom of the Assyrians, which had governed Asia above 1300 years, being dissolved on the death of Sardanapalus, there arose up two empires in its stead; one founded by Arbaces of Media, the other by Belesis of Babylon, the two principal conspirators who brought the former kingdom to an end—Arbaces is in Scripture called Tiglath-Pileser, he fixed his seat at Niniveh and governed his new empire 19 years." Observe, it is erroneously  
*supposed*

Here then with respect to this *Median* kingdom I have pursued that same method, which

*supposed* that he resided at *Niniveh*, for Ctesias expressly says, that he removed the residence to Ec-batana in Media; also that he is made to reign only 19 years, although Ctesias says 28: moreover, that he supposes Arbaces to be the same as Tiglath, only because Tiglath did indeed reign nearly at the above *late* date; but Arbaces himself at least 60 years *sooner*, if he began as according to Eusebius 43 years before 1st Ol.—“Belesis is the same as Nabonassar; he is by Nicolaus Damascenus called Nanibrus [rather Nannarus] and in the Scriptures Baladan, being the father of Merodac or Mordac-Empadus, who sent an embassy to Hezekiah.” He could not well be father of Mordac-Empadus, as there were three other kings between Nabonassar and Mordac, but all 5 reigned 38 years only: neither could Nabonassar and Nannarus be the same, there being 80 years interval between the dates of their existence. These are farther examples of the licentiousness of *conjecture* indulged by all parties on this subject. “*[Ann 739]* Tiglath by planting colonies of Jews in the cities of Media, plainly proves Media to have been then under the king of Assyria; for otherwise what had he to do to plant colonies in Media; therefore Tiglath and Arbaces were not two distinct kings, whereof one had Media and the other Assyria, as Usher supposes, but must both be the *same person* expressed under two different names: and Diodorus positively tells us that Arbaces had *Assyria* as well as *Media* for his share.” This proof of identity is very insufficient, for if Media was subordinate to Assyria, or on the contrary the king of Assyria subordinate to the king of Media, they might in either case by virtue of the command

which I did before concerning the *Assyrian* kingdom, as noticed at p. 80, that is, I  
 X x 2 have

command of the principal king have acted thus without any identity: neither has Diodorus said expressly that Arbaces had *Assyria* for his share, that kingdom not being named, and it is the only thing wanting, which has given rise to so many errors; he has said however, that Arbaces was constituted *omnium provinciarum regem*, which in course implies *Assyria*. Moreover, although Prideaux here makes an identity between Arbaces and Tiglath, yet it does not appear from his words here or afterwards, whether he understood the next Median king Mandaucæ to be the same as the *Assyrian* Salmanassar, and so on down to the end of that Median race of kings. In fact he has scarcely allowed time enough for all the eight kings of Medes; for by fixing Arbaces 29 years after 1st Ol. there remain only 113 years down to the capture of Nineveh by Cyaxares, and this for eight kings: now this sum divided among eight, amounts only to 14 years for a reign. However, although he has left this matter in doubt, yet he appears to have *thought*, that there were *more* kings of the Medes after Arbaces, who reigned in Media, and were the *same* persons as *some or other* of the kings of Assyria in Scripture. For at *Ann.* 721 he writes thus, "Tobit was carried from the tribe of Naphtali into Assyria by Salmanassar; but his brethren *into Media*, Gabael to Rages in Media, and Raguel to Ecbatana, which proves Media to have been *still* under the king of Assyria, and that there was no king in Media *distinct* in those days from the king of Assyria." He must then suppose Salmanassar to be the same as Mandaucæ successor to Arbaces, for this latter if reigning only 19 years, was now dead in the 28th  
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have first of all given a *brief and general* account of the true historic state of affairs, as it appears to myself, together with *some* evidence for it during the interval under consideration; reserving a more *particular* statement of the evidence which supports it to my subsequent pages, in which I shall examine the several events that took place in a more minute manner, and at the same time point out the conjectural *errors* into which other authors have fallen together with the causes of them; and which will be thus rendered the more obvious by such a contrast with the truth of things. These errors however are so numerous and some of them so extravagant, that I should involve my readers in an endless wilderness if I attempted to state them all in any particular manner; I will therefore previously give here a brief view of them only by reducing

of Nabonassar, when Samaria was taken and Tobit carried to Assyria: however, he certainly *mentions* afterwards the three last kings of Assyria in the æra of Nabonassar Assardinus, Saosduchinus, and Chyniladanus to be the same as Esarchaddon in Scripture and two later but unnamed kings of Assyria there; which indeed Usher and many others had conceived before, but none of them supposed those three kings to be the same also as three of the later kings of the Medes in Ctesias, which was the real fact. Hence it is plain, that Prideaux has only just given me a short hint, and assisted me no farther.

ducing them to a few *general classes*, that the nature of them and the objects in view may be the more readily conceived: hence it will appear, that now for the first time at the end of three centuries readers are here presented with such historic evidence only both concerning events and dates, as has actually been preserved in antient authors still extant, without an intermixture with any of the invented opinions of the moderns, by which they have hitherto been misled and the history of this period corrupted.

Now the *first class* of modern plans invented to reconcile the *Median* kings of Ctesias with the *Assyrian* kings in Scripture includes those authors, who have adopted the opinion of Marsham in 1672 referred to at p. 230, that these accounts of Ctesias are almost if not altogether a fabulous fabrication by himself, on which we can place no dependance either with respect to dates or events. This is certainly cutting the question very short, but at the same time in a very hasty, arbitrary, and ill-founded manner; yet a similar example had been set before by no less a man than the celebrated Usher in 1650: for although he does not indeed employ such peremptory words, yet he has done the same in effect,

effect, by having in his Chronology totally *omitted* all notice of those *Median* kings of Ctesias, excepting Arbaces himself, just as he has also totally omitted all notice of the *Assyrian* kings of that author, as if both catalogues were equally fabulous; and in this they have been followed by Newton, Calmet, the Universal History, and others. Now in opposition to this I have already shewn, that we have no foundation for supposing, that the catalogue of Assyrian kings by Ctesias was the invention of himself, it being repeated with more accuracy by Berosus within less than a century after, and pronounced by him to have been received from Asiatic authority; so that Ctesias could have been only a narrator of this fabulous astronomic Chronology invented by others, just as Sir William Jones and others have given relations of a similar artificial Chronology subsisting even still among the Hindoos. It is unreasonable therefore to conclude, that all *later* historic accounts collected by Ctesias in Asia are fabulous, because that of the Assyrian kings is so; for those later ones had never been confined within such astrologic boundaries as the earlier ones concerning Assyria: by a similar way of reasoning we might equally condemn all later Roman history during  
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the republic, because the earlier accounts under the Roman kings are suspected of falsification. Beside this, it appears from other evidence, that the *number* of Median kings in Ctesias is confirmed by Scripture in conjunction with the æra of Nabonassar, and also the dates of *accession* of some of them, together with even the right *duration* of some of their reigns, and especially the date of the *end* in Ctesias of the last of those kings at the capture of Niniveh their capital by Cyaxares; which agrees exactly with its apparent date in Scripture, in the Jewish chronicle *Seder-olam*, in the æra of Nabonassar and in Eusebius. These are facts, which sufficiently prove the above-mentioned moderns to have been at least too hasty in their suspicions, and an ocular view of the truth of these facts may be seen in my annexed Table; to all which they have either never paid any attention, or else in the sublimity of invention they have considered historic evidence concerning dates and facts as mere nothings toward the investigation of truth, when compared with the amusing art of plausible conjectures, or the affectation of superior critical sagacity: as if they were possessed of an infallible innate touchstone, to enable them to distinguish truth from error without the drudgery

drudgery of adducing satisfactory historic evidence in support of their opinions. Of this too much appears in all the writings of Marsham and Dodwell, as well as Scaliger their archetype; and which may therefore be not improperly considered as curious specimens of learned romance, interspersed indeed with some splendid episodes of strong reason, truth and erudition. In all nations both antient and modern, their earliest history is but too much intermixed with fabulous tales of one kind or other, more especially in regard to the antiquity of their existence, yet the more we descend to later times the more veracity we find in them; just so also it has happened in regard to the Median kings in Ctesias, for his dates and durations assigned to three or four of the *latest* of them will be found very accurately stated, notwithstanding the five *earliest* of them are made to begin their reigns too soon, and continue them also too long: to this he was led not by any intentional misrepresentations of his own, but almost necessarily, because the Asiatic Chronology as settled by the arbitrary astrologic period of 1460 had fixed the *end* of the preceding Assyrian kingdom about 50 years too soon, by placing it in the 120th year before Nabonassar, where

where one period of 1460 ended and another began; which made it necessary to distribute those supernumerary 50 years among the reigns of the first five Median kings who succeeded, in order to get right at last near the *end* of that succession, as he has accordingly done.

A *second class* of moderns have however, notwithstanding such suspicions, allowed that the Median kings of Ctesias actually existed, but that they were only a set of deputies or subordinate kings in Media under the supreme kings of Assyria at Niniveh; and that the latter were the Assyrian kings mentioned in Scripture. Petavius in 1627 is at the head of this class; for he *conjectures*, that after the death of Arbaces, the power of his successors declined so much, that the Assyrians set up again as their king one of the posterity of the lately deposed Sardanapalus; so that those Median successors of Arbaces became afterwards only subordinate kings to this new Assyrian race at Niniveh, who were the persons called kings of Assyria in Scripture and supreme kings over Media also as well as Assyria: which he therefore calls a *second* Assyrian kingdom, and not improperly indeed, whether his *conjectures* be true or not; but under which of the



posterity of Arbaces this revolution happened he does not attempt to determine.\* This opinion has been followed by Simpson in 1636, by Usher in 1650, and many other later authors: with this difference however in Usher, that he ventured to determine what Petavius left uncertain, at what time and under what king this second Assyrian kingdom began, viz. with an apocryphal Ninus junior and immediately after the death of Sardanapalus; not *gradually* (*paulatim*) as Petavius suggested. Since Usher has not been followed in this by others it is needless to shew, that he has been himself more fabulous concerning that Ninus junior than Ctesias whom he rejected, therefore Prideaux rejected him.

A *third class* not only agreed with Ctesias, that the Median kings really existed, but moreover that they remained always the *supreme* kings, and that those Assyrian kings in Scripture were only deputies or subordinate kings over Assyria under the Medes; this was the plan of Riceioli in 1669 and of others as well as of Jac. Capellus before him.

A *fourth*

\* " Aliquanto post intervallo exolescentibus *paulatim* Medis defecisse primum quidem Assyrios, postea Babylonios." *Cap. 35. de reditico Assyriorum imperio.*

A *fourth class* is formed by the singular plan of Jackson, who does not indeed conceive the Median succession in Ctesias to be fabulous, but only that Ctesias has made great *historic mistakes* concerning it, particularly “ by supposing Arbaces to have destroyed the Assyrian kingdom sooner than the real truth, which event he says, did not happen until long after, for under the 5th king in his catalogue Arbaces, the Sennacherib in Scripture, the Medes first revolted; so that the four preceding Median kings in Ctesias were only deputies under the *first* Assyrian kings who ended with Sardanapalus, and who was the same he *supposes* as Assardinus, at the *beginning* of whose reign the Medes having revolted and after having lived a few years in a republic, as mentioned he says by Herodotus, they chose Dejoces for their king: so that Ctesias by an *unaccountable mistake* has placed Sardanapalus and Arbaces 100 years too soon, and made a prior dissolution of the Assyrian kingdom by Arbaces 200 years before its real and first destruction by Cyaxares; Artæus is thus made to be the same person as Dejoces, after whose accession it was that the remaining Median kings of Ctesias ruled in Media, and Assardinus and his successors

in Assyria, until Niniveh was captured by Cyaxares, and thus the Assyrian kingdom totally destroyed for the *first* time. By this plan the Assyrian kings in Scripture were the same persons as the five or six latest kings of the Assyrian kingdom *before* Sardanapalus *i. e.* Esarchaddon, there being thus only *one* Assyrian kingdom, and thus Sardanapalus not beginning to reign until 69 years at least *after* the first Olympiad." This is all so fanciful and destitute of evidence as to deserve no farther consideration, than just to observe how the spirit of conjecture and invention increases when writers once begin to give way to it, instead of strictly confining themselves to the evidence transmitted to us from ancient authors.

There is still a *fifth class* of moderns, whom we may call *the obscures*, such as Jac. Capellus, Calvisius and others, who have indeed mentioned and fixed the reigns of some of the Median kings of Ctesias, but omitted others, and who neither decide whether they governed in Media only or in Assyria or in both, nor whether they were the same or different persons from the Assyrian kings in Scripture; but have left their readers to find out all such facts as well as they can, contenting themselves merely



merely to fix the dates when they are *supposed* to have lived, if they ever lived at all. Of all these *five* classes therefore we may truly say, that in the very moment, while they are supposing Ctesias to deal more or less in antient fables, they have proved themselves to be altogether inventors of modern fables, with the advantage however of having supplied us with a greater variety of them.

A *sixth class* was sketched out by Prideaux, viz. that the first five or six kings of the Medes in Ctesias were the very *same persons* as the first five or six kings of Scripture; this it is my object to support, by shewing it to be the only one which is consistent with the remains of historic evidence now existing; and as this is indeed very little, by pointing out still farther what hitherto has escaped the observation of all others, that a great number of dates, sums of reign and computations of *time* have been preserved, although almost all historic evidence of *events* has perished; but that the former will happily serve in many cases as supports to the latter, by proving the identity of several of those kings by means of the identity of the times of their *accessions* and *deaths* and *kingdoms* possessed by them, instead of the identity  
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of their *names, titles and actions* : with this view it was, that my annexed Table of Dates was compiled, that readers may there instantaneously see with their eyes that harmony of chronologic dates which by means merely of words they can only learn successively after adding page to page, and even thus with less precision. That this *sixth class* of opinions, which not only appears true, but also to be the most obvious, should be thus among the latest of those adopted, may at first seem rather extraordinary ; but it has happened in this as in many other cases that more simple solutions of difficulties are often overlooked, while men are intent upon discovering such as are more complex ; so that truth is thus found to lie sometimes not so deep in the well, as too many are apt to suppose. I have however myself erroneously mentioned before, that no authors in foreign countries had adopted this opinion of Prideaux ; for I now find that his plan has been in part followed in the *Système Chronologique de la Bible, par Michel, Toul, 1742* :\* and it has been more lately

\* *Michel* however differs from *Prideaux* in several material points ; for he places *Arbaces*, like *Ctesias*, 91 years before 1st Ol., and not at the epoch

lately adopted by Faulkner in his *Chronology at Oxford*, 1796, with whom I had some correspondence on this and other subjects, to which some reference is made in his *Prefatory Discourse*; and he has likewise adopted my dates of the accessions of the Jewish kings, but he appears to have been undecided concerning some other particulars, which I suggested to him; whereby in regard to these latter he has gone on in the old discordant opinions. Moreover, although in his Advertisement he professes, that his work was undertaken *with a view of adjusting the reigns of Jewish kings to those of Eastern monarchs*, yet he has too superficially mentioned this subject of the Assyrian kings in Scripture being the *same persons* as the Median kings  
in

epoch of Nabonassar; he also supposes *all* the eight kings of the Medes to be the same persons as *all* kings of Assyria in Scripture, just like myself; but Prideaux has left this uncertain. Yet in this Michel differs from both of us, for he *supposes*, that the successors to Arbaces in Ctesias were in reality not his posterity, but the posterity of Sardanapalus re-established, and thus Assyrians by nation, though kings of the Medes as well as of Assyria: so that Arbaces did nothing more than re-establish the privileges of the Medes, and an *autonomy* i. e. a right of making their own laws; in all other respects they still as before continued subjects to the kings of Assyria mentioned in Scripture. P. 236.



in Ctesias, without entering into any *proofs* of what Median kings had an identity with what Assyrian kings except by conjecture. This is an omission of one of the most material articles in such a subject, which therefore has rendered his work as imperfect in this particular respect, as it is erroneous in its general plan of determining the dates of events by an average of the years commonly contained in the generations of men, which he fixes at 24; a very precarious method of computation, and one which nothing but the great name of Newton could have ever introduced as rational! It may indeed be resorted to as a means of approximating to truth in those cases, where history has transmitted to us no evidence whatever concerning dates and times; but can never with any shadow of reason be made use of as by Newton and Falconer to oppose and displace dates of events, when the historic testimony to them is positive and harmonious, and no way improbable; nay if it be not harmonious, yet a medium between such different dates will come nearer to truth than any average of generations, in case there be no obvious reasons why one testimony should be preferred to some other. In this present inquiry then I propose to accomplish what  
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has never as yet been sufficiently performed notwithstanding any professions of it, that of proving that the Jewish Scriptures in their historic accounts do in reality agree harmoniously with the *only* and few profane accounts of the same Assyrian reigns, which have been transmitted down to modern ages ; and that by accident this is more discoverable with respect to *dates* and *times* than in regard to *names* and *actions*, or other circumstances in the reigns of the kings in question. Thus Chronology will be rendered useful to supply the deficiency of *history*, just as in my commentaries on the Jewish prophets we found it sometimes useful to supply the defects of antiquated *language* and corrupted manuscripts ; as also with respect to profane authors to become sometimes a critical expositor of the *meaning and integrity* of many passages which occur in them, and which rash editors have altered for a worse text, because they did not understand them.

But it may be acceptable still farther to those, who have not much attended to this subject, if instead of that harmony of *Chronology*, which they will perceive in my annexed table, I point out to them moreover, that the modern accounts have

on the contrary rendered the *Chronology* of those times, if possible, still more uncertain, variable and contradictory than their *history*; so that they sometimes differ to the extent of 200 years, and this among those, who all profess themselves friends to the veracity of Scripture. It is however true, that the antient accounts themselves differ in regard to the *commencement* of the kingdom of Medes to the amount of 50 years, the cause of which I have pointed out; but none exceed that except Justin, the origin of whose peculiar error admits also of some probable explication, as being the consequence of Trogus the original author having misunderstood some parts of the abridgement of Ctesias by Diodorus: which is indeed by its too great brevity, ambiguity and obscurity so liable to misconception, that several of the most learned moderns have in like manner understood his account both of the number of Median kings and the duration of their reigns in very different senses from one another, and differently also from Trogus; but yet not any one of them seems to have perceived what the true sum is, which arises from all those reigns when properly added together, and which when reckoned backward serve to fix the *commencement* of that



that kingdom according to the real meaning of Ctesias. This I shall shew to be the 307th year before Cyrus as by Polyhistor, the 91st before the 1st Olympiad; but by Trogius in Justin 43 years sooner, which error seems to have arisen from an ambiguity concerning the sums of Ctesias in Diodorus.\* Eusebius however, and Syncellus after Herodotus make it only 40

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\* It is very possible, and probable, that Trogius might have read the *Universal History* of Diodorus in Greek before he finished his own in Latin. Dodwell indeed says, that Trogius lived before Diodorus, but this is a mistake, *Diodoro antiquior Trogius* [Dissert. 9. p. 372]. For at the end of the 43d book Justin mentions, "that Trogius himself had said in his history, that his father served in the wars under Julius Cæsar." Now the history of Diodorus ends at the 731st Olympic year, which was the 45th before Christ, but he published his work at a later date, for he calls Julius Cæsar *Divus*, who died in the following year the 44th, yet how much later is not known. So that the father of Trogius and Diodorus were partly cotemporaries, and Trogius himself a generation younger. At what age he compiled his history is not known, but it must at least have been later than the 20th before Christ; for Justin in his abridgement of the 42d book relates as from Trogius the conquest of the Parthians by Augustus and the recovery of the Roman eagles lost by Crassus: this event happened in the 20th before Christ, 10 years after the victory over Anthony at Actium. There is good reason then to presume, that Trogius had seen the history of Diodorus,

or 43 years before 1st Ol.\* These are the chief differences among the antients concerning the date of this event, but those of the moderns are so many and so vast, that Vignoles rightly says " Scarce any two moderns agree together in the same date, and one's patience is lost in comparing their dissensions." However I will enumerate some of them, that readers may be the better satisfied of the necessity of settling this date in a more accurate manner, instead of being for ever tossed about upon the ocean of arbitrary and contradictory conjectures.

Now Scaliger was wavering concerning this date, but did not omit it, as Petavius unjustly accuses him † he fixes it in the 282d before Cyrus, which was 66 before 1st Olympiad [*in edit.* 1629. p. 42. of *Fragm.*], but in his *Euseb. apud ann.* 1171. at 97, yet in both derives it from Ctesias

\* See p. 143 of *Illustrations*; and in another place he again makes it 40, for he says, that it was 96 years before the capture of Samaria by Salmanassar, which being in the 28th year of Nabonassar, on deducting 27 the remainder is 69, and deducting again the 29 years before the epoch of Nabonassar after 1st Ol. there remain 40 before 1st Ol. *Sync. p.* 205.

† " Epocham hanc Medorum omisit Scaliger," *Doctr. temp.* 9. 34.

Ctesias [idque Ctesiam hausisse ex archivis Persicis] which shews the account in Diodorus to be so dubious, that even the same person did not always reckon up his sums to the same amount, much less different persons. Petavius from the same authority of Diodorus's abridgement of Ctesias makes them amount to 100; and Marsham to 141 before 1st Ol. He has not indeed included in his total *all* the reigns mentioned by Ctesias, and has thus left his meaning as to the total sum in some doubt; but so far as he does express clearly his conception of the right method of reckoning up those reigns in his *table* under the article of Ctesias, he could not *mean* less than 141, which are 7 years more than in Justin. The cause of this excess is, that he reckons up one whole reign of 40 years for *Artibarnas*; which Petavius omits, as conceiving him to be the same with the subsequent king in Ctesias *Astibaras*, in which he was however apparently mistaken. These different conclusions deduced from the same *profane* historian Ctesias were only the beginning of sorrows; for while the above moderns, who thus followed Ctesias and Justin, differed so greatly in their dates and sums, another set rather proposed the 40 or 43 years of the

*Christians*



*Christians* Eusebius and Syncellus before 1st Ol. as their guides to the right date. This was the date in Funcius, Jac. Capellus, Simpson and others. Calvisius about the same time went back to the 97 of Scaliger, Emmius reduced it to 77, but Helvicus augmented it to 95, and Riccioli again reduced it to 87. Cary augmented it to 100, but Pezron still more to the date of Justin 134, or rather 114; because he makes the Median kingdom to *end* 20 years later than those others, that is at the capture of Babylon by Cyrus 20 years after his accession to Media and Persia. Vignoles however adopted the full sum of Justin 134 before 1st Ol. and in order to make up so early a date he has interpolated one whole reign of 33 years not mentioned by Ctesias, which being deducted reduces his date to 101. From these discordances we may well conclude, that there must be some great ambiguities indeed in the sums of reigns by Ctesias, since all these authors, who exceed the 43 years of Eusebius, profess to deduce their different dates from the sums of the same historian Ctesias. This renders it expedient for us to examine in what those ambiguities of Diodorus relative to the account of Ctesias consist, and how to steer a  
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right course among them ; in which we shall be greatly assisted by our new guide Polyhistor, whose date of the commencement of the Median kingdom has been fortunately quoted by Agathias : to this however not any one of the moderns has hitherto given the least attention, although it is very important, not only on account of its greater precision, but also as being exactly the same, as what we shall find to arise from the sums of reign by Ctesias, when they are rightly reckoned backward from Cyrus ; which right mode of reckoning them the words of Diodorus will not only *allow*, but in reality do rather seem to *imply* than otherwise. Thus Polyhistor will become an arbitrator between the different opinions of the moderns concerning the meaning of Ctesias and Diodorus ; whereas in fact they seem to have rather considered consistency with other antient accounts, as a matter of no moment in their interpretations of the words of those authors, nor as a means of determining what otherwise might be doubtful. So that in this as in many other cases already mentioned we shall find nothing but harmony to subsist, where others have found nothing but discord ; a discord however of their own creation only, arising from their  
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own mode of interpreting the too brief information of Diodorus; which has indeed some ambiguity in it to us who are ignorant of the history of those times, but the deficiency of explicit information here had been possibly supplied by more full information on this subject in those books of Diodorus, which are lost and contained his own history of those times at length instead of this epitome of Ctesias: and in which he may have *preceded* Eusebius in fixing the commencement of Arbaces, like Herodotus, at only 40 years or more before 1st Ol., instead of the 91 of Ctesias and Polyhistor as computed in that epitome.

But there is *another class* of moderns, who have neither adhered to the 91 of Ctesias, nor yet to the *later* date of 40 in Herodotus, Eusebius, Syncellus, and possibly in Diodorus likewise, but have invented new dates of a *still later* kind out of their own imagination and without any evidence whatever from antiquity.\* One or

\* At p. 143 of *Illustrations* I have erroneously said, that the date of Syncellus was 50 before 1st Ol., as this is fixed by him, which is indeed 10 years too late; therefore that it would be only 40, if his 1st Olympiad had been rightly fixed 10 years more early;



or other nearly of the foregoing two dates all moderns had adopted until Usher first introduced a new opinion, which placed the dissolution of the Assyrian kingdom at a *later* date than the 1st Olympiad instead of a century more or less *before* it; for he supposed Arbaces in Ctesias to be cotemporary with Nabonassar, with whom began that celebrated æra and succession of kings at Babylon, and whom he supposed to be the same person as the Babylonian Belesis, who was assistant to Arbaces in dissolving the Assyrian kingdom. This produced additional obstacles to the perception of the Assyrian kings in Scripture having lived *in the same times* as the first kings of the Medes; for instead of the former error of those Median kings having *preceded* the Assyrian ones by 50 years, they were thus made to *live later* than those Assyrian ones by 50 years and more; Arbaces being thus made cotemporary with Tiglah the third Assyrian king in Scripture, whom Usher supposed to be the same person as a Ninus junior mentioned by Castor in Syncellus (*p.* 205) as succeeding Sardanapalus

VOL. III.

3 A

danapalus

early; but I now find, that I ought to have made the sums to be 54 and 44. Possibly also his error in the date of 1st Ol. may be only 9 years instead of 10.

danapalus in Assyria, his predecessor Phul being the same as Sardanapalus himself. Thus this Ninus or Tiglah were the founders of a subsequent and *second* Assyrian kingdom at Niniveh, while Arbaces and his successors contented themselves with Media only and his assistant Belesis, i. e. Nabonassar with Babylon; so that the revolution under Arbaces occasioned only the death of Sardanapalus, the Phul of Scripture, and the separation of his dominions into three different kingdoms and three different successions of kings. This totally unhinged all former dates and computations, by thus fixing the dissolution of the Assyrian kingdom at 120 years *later* than Ctesias and 70 years later than Herodotus, Eusebius and the first Assyrian king in Scripture: yet this eccentric conjecture was however adopted by Prideaux, Calmet, Universal History and others, except with some slight alterations; although there is not the least antient testimony extant for *so late* a date, except a very doubtful one in Paterculus, nor any historic account in antiquity, which he could follow as his archetype, except a confused incoherent plan in Syncellus. For though Syncellus had rightly mentioned in one passage, as quoted before, that the *same persons* might

might be not improperly sometimes called kings of Media and sometimes kings of Assyria, since they reigned over both nations (which truth he probably copied from some other author) yet he himself nevertheless supposed Arbaces and his successors in Media to be a *different* succession from that of Phul or Tiglah in Scripture; but that Salmanassar however was the same person as Nabonassar and both of them kings of the *Chaldeans* not of Assyria. Salmanassar and Nabonassar did indeed live *in the same times*, yet one in Assyria, the other in Babylon; so that his chronology was right, although his historic account was erroneous: but it was reserved for Usher, otherwise deserving every praise, to err both in point of *time* and *history*, by bringing down the reign of Arbaces so late as to Tiglah, the predecessor of Salmanassar; and by making also Tiglah to be the same person as Ninus junior a pretended successor to Sardanapalus. Thus the moderns have run from one extreme to another; and by so doing have rendered it impossible for readers to discern, that the kings of the Medes in profane authors were the *same persons* as the kings of Assyria in Scripture, or even that they were *cotemporary*, although this truth has been all along close



under their noses, and even in the mouths of some of them. This shews the dangerous nature of mere conjectures when supported by no antient evidence; and the still greater danger arising from highly esteemed authority, which is thus able to render current and respectable such rash opinions as are inconsistent with all evidence, probability and reason:\* yet even with

\* The only date favorable to this opinion is a very doubtful one in Paterculus, which places the dissolution of the Assyrian kingdom by Arbaces at DCCLXX, *ann. ante consulatum Vinicii*. Now Vinicius was consul in the 30th after Chr., which being deducted leave 740 before Chr. for the date of that dissolution; but this was 7 years later than the epoch of Nabonassar in 747 and 36 years later than 1st Ol. Hence Lipsius conjectured that one century or one C was lost out of the date by the copyists, which would make it 870 (DCCCLXX), therefore 64 years before 1st Ol. This was approved by Scaliger, and Ruhnkenius in his edition of Paterculus, but neither of them produce any evidence to countenance their approbation of this date (*v. Scal. Can. isag. lib. 3.*) and it seems to me clear enough, that Paterculus rather meant to place Arbaces at a full century before 1st Ol.; so that the original date could not be less than 920 or 910 [i. e. DCCCCX] which would be 114 or 104 years before 1st Ol.; he therefore followed Ctesias. For after saying, that *Arbaces dissolved the Assyrian kingdom*, he immediately adds in the very next sentence *Ea ætate fuit Lycurgus*. Now no antient ever placed Lycurgus at less than a century before 1st Ol.;

Newton

with this only doubtful date, to which Usher could appeal for support, he does not agree sufficiently; for he should by that have placed Arbaces at 36 years after 1st Ol., and not at 29, the epoch of Nabonassar. The difference of 7 years is not indeed

Newton indeed makes him later, but the very evidence, which he brings for it proves the directly contrary: Eusebius places him at 110 before 1st Ol., and Syncellus makes him cotemporary with Iphitus, therefore not much less and possibly more (p. 196). Paterculus indeed proceeds to mention 3 or 4 other events at not more than 44 and 24 years before 1st Ol., but then he changes his phrase from *ea ætate* to *hujus temporis tractu*, which latter seems to imply no more than *in the subsequent course of that age*. Yet at least all the events there mentioned are placed by him before the 1st Ol., and not after it, so that the date of Arbaces also must be before 1st Ol., otherwise it could not have been in *ea ætate* and *hujus temporis tractu*: moreover as this date is the first of them there mentioned, it probably was the most antient, and he thus uses *ætas* and *tempus* as synonymous to the century before 1st Ol. Usher therefore could derive no support from this date, neither indeed does he pretend it; but as he does refer to Paterculus for the name Pharnaces instead of Arbaces, if the date there did not influence Usher, yet it may influence others until they have recourse to those subsequent sentences and dates which I have quoted, and which prove that 100 years before 1st Ol., was rather the meaning of the author than 36 years after it, and it is certain, that we must seek for his meaning from his concomitant words and not the date now there.

indeed considerable, yet it affords another probable proof of the date being corrupt; since the epoch of the æra of Nabonassar was so well known and celebrated among the antients, that we cannot well suppose, that Paterculus could be ignorant of it, if he had meant with Usher and Prideaux that Nabonassar was the same person as either Arbaces or his associate Belesis. Prideaux, Calmet and Universal History have adopted a few variations from Usher in the *historic* part but not in the *chronologic*, except that Calmet says, this Ninus *i. e.* Tiglah reigned 30 years, and yet he makes Tiglah die 19 years after, like Prideaux; thus these authors neither agree with themselves nor with antient accounts, but alter every thing by conjecture at their own pleasure. This is not history and chronology, but only modern fable.

However, notwithstanding this authority of Usher in 1650, yet Hyde afterwards in his *Rel. Persar.* 1710, still adhered to the date of 100 after Ctesias, as did *Loyd's tables* and others soon after to the 43 only of Eusebius, Jac. Capellus and Simpson before 1st Ol., in the *middle* between those extremes; this made Arbaces and the first Assyrian kings in Scripture at least live *in the same times*, although they had no conception



ception of any *identity* between them any more than Eusebius. But this shews with what difficulty modern authors were at first drawn away from the opinion of those kings living in *the same times*, and in the *middle* between the two extremes adopted by others, of a whole century either *sooner* or *later*, which is the first step toward their identity; and which former opinion therefore no person can consider as an innovation by myself, since it has in reality been the uniform opinion of many modern as well as all antient Christians, who were not misled either by the astrologic date of Ctesias on the one hand, or the eccentric and equally fabulous conjecture of Usher on the other, which has been since augmented to still greater extravagance and error by Newton and Jackson. For Newton brought down the *beginning* of any Assyrian kingdom to 30 or 40 years *still later* than Usher's *end* of it, or Jackson's, which is as late though for different reasons, i. e. 200 years later than Ctesias and Justin. Newton indeed attempted to justify this upon pretendedly scientific principles, but Jackson only depended upon his own *supposition* of a pretended *unaccountable historic mistake* made by Ctesias, and not upon those principles adopted by  
 Newton.

Newton. But since his time Mr. Falconer has once more adopted the principles of Newton, a little varied however, in his *Chronology* at Oxford; and upon the foundation of those same principles has carried back again the dissolution of the Assyrian kingdom by Arbaces to be nearly *cotemporary* with 1st Ol., a novelty never yet started by any other person, either with respect to the date or to the principle on which it is founded: this is however still 40 years too late, for thus Arbaces is made to be *the same* as Phul, not as Jarib; and with this *latest* system of chronology the question rests at present. Such is the confusion uncertainty and dissention between these latest Christians, after their having deserted all historic testimony and the authority of the antient Christians, in order to depend solely upon their own better sagacity and the good fortune of conjectures: if any of them have at last arrived near to the truth, yet it must at least be confessed, that they have taken a very long round about road by a circuit of 200 years in order to arrive at it; but we do not as yet find, that they have come to any agreement at last among themselves, therefore probably not with truth either. Usher and Prideaux gave to Arbaces only a small jump of near 100 years;

years; but Newton entirely discarded Arbaces and his successors as non-entities like his tutor Marsham; yet Mr. Falconer though a disciple of Newton has brought them all to life again, and this sport they call *the connexion of sacred and profane history*. Unfortunate indeed would be the fate of Scripture, if its veracity depended on such conjectures as these for its agreement or otherwise with profane history; which can only lay a foundation for the cavils of sceptics, without carrying any conviction along with them in the minds of critical and discerning readers!

The moderns therefore have plainly made no improvement in the chronology of the times in question, although they have indeed in many other cases, and although they have been continually shifting their ground during an interval of 300 years: and the above historic account of their variations not only shews the nature of them, but also the causes of their errors; which may be thus more briefly expressed in a summary way. The Jews caused the first error of making the Assyrian kings in Scripture live *too soon* to be the profane kings of the Medes, by erroneously placing their own Jewish kings in a too early age: the first Christians followed and even in-



creased their error. But Eusebius from the information of profane historians fixed the kings of the Medes right, and also so much reduced the Jewish error, though not by right means indeed, that these two successions were made *nearly cotemporary*. Jac. Capellus and other early moderns followed Eusebius: but Petavius chose a new guide in Ctesias, whereby he caused the contrary error of placing the profane kings of the Medes *too soon* to be cotemporary with the Assyrian kings of Scripture, and many others followed his authority. But Usher fell into an opposite error, of fixing the kings of the Medes so much *later* than Eusebius, that the Scriptural kings of Assyria lived again *too soon* for them, so that the two successions could not be cotemporary. Simpson however, Loyd and others soon after revived the truer plan of Eusebius; yet still it could not continue: for Prideaux afterwards adopted the opinion of Usher and misled the *Universal History*, although at the same time he first started a *hint* of the real truth; so perplexed however by his other errors, that nobody hitherto gave any credit to it, when he affirmed the *identity* of some of the kings of the Medes with *some* of the Scriptural kings of Assyria, though not with all of them.

them. This error, which was only unfounded conjecture in Usher, Newton pretended to confirm upon scientific principles, and also increased that error of placing the kings of the Medes *too late*. Jackson rather augmented the same error, yet induced to it however by different reasons; but Falconer has since lessened the above error upon the foundation of that very principle of Newton which first confirmed it; and thus these variations have ended with still making the first king of the Medes live *too late* by 40 years to be *cotemporary* with the first Assyrian king of Scripture and in Eusebius; but to be at least *cotemporary* is the first necessary step toward a proof of their *identity*.—Upon the whole then it is evident that I advance no novelty in finding the Scriptural and Median successions in Assyria to have been *cotemporary*; for I thus only recover the ground, which has been almost lost since the age of Eusebius, by the attachment of Peta-vius to the *astrologic* computations of Asia made known by Ctesias, by an unfounded *conjecture* of Usher in opposition to all history, by an equally unfounded *principle* adopted by Newton, and by a pretended *historic mistake* discovered by Jackson. So that as Vossius says, “Sciendum est nos

non hic novam condere chronologiam, sed veterem temerè abrogatam reducere." *De ætat. Mundi, p. ult.* : and the same I have already shewn to be the fact in several other cases. I mention this because although novelties often please, yet they as often become the foundation of objection ; and the public through the above-mentioned causes has now been so long in the road of error, that it implies some appearance of novelty to bring them back once more into old and forgotten opinions more consistent with truth. For Eusebius had long ago fixed the beginning of the Median kingdom under Arbaces in the 19th of Jereboam the 2d. which 19th was placed by him only about 12 years *too soon* : and if he had not followed the septuagint MSS in his time by giving to Amon 12 years instead of the 2 only in the Hebrew text, which raised up Jereboam *too high* by 10 years, he would perhaps have been exactly or nearly right in fixing the dates of both the Jewish and Median kings, who would thus have been *cotemporary*. For Arbaces would have thus commenced a few years after Jereboam's accession, and soon *after* Jonah's prediction of the fall of Niniveh at the commencement of Jereboam ; for although after the first attack of



of it by Arbaces it was rescued from him for 3 years, as Diodorus relates (and this as Jonah says on account of its repentance) yet at the second attack he succeeded: in this reign also lived Hosea, who mentions *Jarib*, that is, Arbaces as being then on the throne of Assyria or at least *before* he prophesied concerning subsequent events relative to Judah and Israel in the same reign of Jereboam. Thus all these events correspond at these dates most harmoniously with one another, and with profane authors as well as with Scripture; which the moderns by their erroneous dates have nevertheless involved in confusion and contradiction by adopting some other plan than this of Eusebius, who laid a good foundation, although he did not raise upon it altogether an uniform building.\* But Syncellus

\* Neither here again can I claim novelty with respect to the *harmony* caused by these dates, although I may indeed afterwards in regard to the *proofs* of those dates, which produce this harmony; for Jac. Capellus had long ago noticed the harmony arising from these dates adopted by Eusebius and himself. “Sub Jereboam Jonas Ninivitis excidium denunciavit, sed sub conditione nisi resipiscerent, certè post Jonæ prophetiam Ninivitas resipuisse legimus [and accordingly Ctesias says that after the first attack by Arbaces Niniveh escaped until the second siege 3 years after] complementum ergo prophetiæ

Syncellus still improved upon it, for after correcting the error of Eusebius by reducing the 12 years of Amon to 2 only, he fixed the accession of Arbaces upon the 6th of Jereboam, which was either exactly right or not far erroneous.\* Thus I have not only shewn what the *different classes* of error have hitherto been, which have prevailed in modern times, but also what gave origin to them; and that when these errors are corrected, we necessarily return back once more to the chronologic system of Eusebius and Syncellus, which made Jarib and Phul in Scripture *cotemporary* with Arbaces and his successor kings of the Medes: now this affords a *presumption* at least, that they might be the *same persons* under different names, and more so if they reigned over the *same countries* at the very *same time*, as I shall hereafter prove more minutely. Notwithstanding however the above *different classes* of error the authors of some of them have nevertheless approached *near* to the true *historic* date of the commencement of the kingdom of Medes

prophetiæ Jonæ potest referri ad expeditionem Arbacis adversus Sardanapalum; sed Nahumi prophetia contra Ninivitas ad bella Cyaxaris adversus Assyrios." *Apud ann. 834 ant. Chr.*

\* See *Illustrations* p. 150.

Medes at about 40 years or more before 1st Ol.; and others *near* to its date in the *artificial* Asiatic period, namely 91 years before 1st Ol., but none of them have *exactly* fixed upon those right dates, nor yet produced sufficient evidence for the dates which they did adopt; but they have sometimes sacrificed positive antient testimony both profane and Christian to pretences of better critical sagacity in themselves, and sometimes to a servile respect for the authority of favorite modern authors, or else to a too great self-confidence in novel theories of their own invention. So that it has fallen to my lot at last after the course of 3 centuries to enquire what antient evidence has been actually preserved concerning this subject, and to rest my opinions altogether upon that; but I soon found, that such evidence necessarily brought us back to the statement of Eusebius and Syncellus, that the first Assyrian kings in Scripture were *cotemporary* with Arbaces and the first kings of the Medes: and that still farther antient evidence existed also, which tended to prove the *identity* likewise of those cotemporary kings agreeably to the *hints* given incidentally first by Syncellus and not long since by Prideaux. Hereby we are now at last after so many revolutions



revolutions of opinion enabled to substitute *real history* and *harmonious chronology* attested by antient evidence in place of the many incoherent rhapsodies of modern conjectures; the effect of which will be, that the accounts of Scripture will coincide of themselves harmoniously with the accounts of profane authors. This is a desideratum which has been long wanting, for although the different modern authors of a connexion between sacred and profane history have all affirmed that their own statements agreed with Scripture, yet readers could never hitherto find it; nor easily give credit to assertions by which some authors placed the same events at the distance of 200 years from the statements of others. This diversity of errors was indeed through ignorance begun by the Jews and first Christians, but the chief of their errors have been corrected by the moderns; yet 3 or 4 years of them still subsisted in the dates of the Jewish kings, which although but little in themselves, yet would produce this ill consequence, that the computations of profane authors would differ from Scriptural accounts to that very amount. Thus for instance it may be seen in my table, that if the *commencement* of Arbaces be fixed agreeably  
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to the date of Ctesias, *his end* of the kingdom of Medes will fall in the 141st year of the æra of Nabonassar, the very year or at most only one year sooner or later, than the *end* of the Assyrian kingdom actually happened according to Scripture, Eusebius and the Jewish chronicle Seder-olam ; for this was in the 1st of Jehoiakim, and in case the above 3 or 4 years of error be corrected, the very year of reign then in which the *Seder-olam* fixes that capture of Niniveh by Cyaxares. Now as so little *historic* evidence has been preserved, it is fortunate, that some *chronologic* evidence here comes in as a substitute for the former. This shews the expediency of having in my first and second volumes and in my table at p. 216 vol. ii. corrected this remnant of Jewish error, in their having placed their kings *too early*; of which error 3 or 4 years was still left uncorrected in modern tables of chronology, and which although but little, yet would prevent the above *exact coincidence* between Ctesias and Scripture with respect to the *end* of the Assyrian kingdom according to Scripture falling in the very *same year* as the *end* of the kingdom of Medes in Ctesias : this is an important fact which has never been hitherto noticed by any modern author whatever,

any more than the coincidences in the accessions and deaths of the three later Assyrian with the Median kings, who immediately preceded the capture of Niniveh by Cyaxares. If any person can ascribe these to mere accident, and not as the necessary consequence of the identity of those two successions, I presume, that he will have no objection to ascribe the formation of the universe to the fortuitous concourse of atoms: but if it shall appear still farther, as it assuredly will, that the above three kings of those two successions *began* and *ended* their reigns during the very same time over the very same three kingdoms of Assyria, Babylonia and Persia; I then doubt, whether even the fortuitous system of Epicurus can defend them against such a manifest proof of identity. This *supplement*, which *chronology* thus affords to the scanty materials of *history* now remaining, has never hitherto occurred to any of the moderns, and many traces of it will be seen in various other examples beside those above-mentioned: the foundation moreover on which this evidence rests, has even been already adopted by Mr. Falconer, that is, the dates at which I have fixed the accessions of the Jewish kings; and B. Horsely likewise has evidently though



though silently approved of the same in the Preface to his Commentary on Hosea\*. I have now pointed out still farther the important *consequences* arising thence, by their producing such harmony in other dates as supplies the place of that *historic* information, which can never be now recovered. If it should be objected against this *chronologic* evidence, that although it may thus point out unexpected harmonious coincidences near the *end* of the kingdom of Medes, yet it is quite otherwise at the *beginning* of it; for not only Justin, Polyhistor, Ctesias, Marsham and Petavius differ greatly from one another in their dates of the commencement of that kingdom, but also differ still more from the *later* dates of Herodotus, or of Eusebius, Jerom and other Christians, who again differ from one another, this is indeed true; but it affords no objection against the force of such chronologic evidence, where we can obtain it, that it is unfortunately not to be obtained in every case: moreover, *in general* it is in all cases less attainable at the beginning than at the end

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\* "I am persuaded that Hezekiah's accession was 3 years later than at Usher's date of it." P. 5. This is the very date proposed by myself, and never hitherto by any other.

of empires; but in the present case there is an accidental and *particular* cause likewise which operates in our favour at the *end* of this kingdom of Medes, and which is, that by the three last of those kings becoming kings of Babylon, the dates concerning them have been admitted into the excellent æra of Nabonassar and thereby preserved with accuracy. I have shewn likewise, that there is also another *particular* cause, which accounts for the principal variations at the *beginning*; and as to the lesser variations between Justin, Ctesias, Polyhistor and others they may be all equally well accounted for, by their being different, only because they may be different methods of understanding and computing the accounts found in the same author Diodorus, which in some cases are certainly ambiguous: of this we have a sufficient example among the moderns themselves, for from the same account by Diodorus Marsham reckons up his sums, so as to make the commencement of the Median kingdom be at 141 years before 1st Ol.; while Justin makes it to be only 134, Polyhistor 91, and Petavius 100. The lesser variations also between the antient Christians Eusebius, Orosius, Jerom and others concerning this event

event probably arose from a similar cause, namely, that Hellanicus, Herodotus, or whoever was the historian whom they consulted, had not fixed with precision the very year at which he supposed the Median kingdom to commence; but had only given such an account as enabled others to collect the year and date in question out of the relations by that author, just as is the case now in the abridgement of Ctesias by Diodorus; out of which same relation, Justin, Petavius, Marsham and others have collected very different sums and dates: hence while some antient Christians collected the date to be 64 before 1st Ol., others found it to be rather only 54 or 44, 43 or 40; such small variations however afford no foundation for objection to the chief fact, that the date of the commencement of the kingdom in question was either the very same or nearly the same as some or other of the above-mentioned, that is somewhere between 60 and 40 before 1st Ol. This is still quite sufficient for our purpose of proving, that the first Assyrian king in Scripture lived *at the same time* as the first Median king of profane authors according to the best attested *historic* accounts of their accessions; which are at the same time although very near to one another,



another, yet all so very distant from the 134, 100 or 91 of Ctesias and Polyhistor, as proves them to rest upon the authority of two different computations, one Asiatic, and evidently accommodated to the astro-nomic and current period there of 1460; while the other was apparently traditional and collected from the accounts of the Greek colonies in Asia. To this latter we have found, that *some* Greek reducers of the Assyrian period of 1460 had acceded likewise, for on their having at first cut off 162 years from it after Teutæus and the Trojan war, it was then found, that they had cut off too much, there being only 314 left remaining; therefore they were forced afterwards to carry back the Trojan war to the preceding reign of Tautanes 51 years *sooner*: for, these added to 314 made up 365 years from Troy to the *end* of the Assyrian kingdom, and consequently left only 40 years from that event and the commencement of the Median kingdom down to the 1st Olympiad; thus making up 405 in all from Troy to 1st Olympiad; in which sum of 40 they agreed nearly with Herodotus.\* This union then of so many antient

\* From my statement of the computations of Herodotus

antient *historic* testimonies, some profane  
and some Christian, to the *cotemporary ex-*  
*istence*

Herodotus at p. 113 and in the preceding ones it followed, that he could not have placed the *end* of the Assyrian kingdom *sooner* than 40 before 1st Ol.; but in my *Illustrations* at p. 147 I have shewn, that in case Herodotus adopted some or other of the *more antient* dates of the events in question, and not the more correct ones of the *later* Greeks, he then might by such means have placed that event and the commencement of the Median kingdom 10 or 20 years possibly sooner, that is, 50 or 60 years before 1st Ol. Now since this I have discovered, that he actually did adopt *one* of those more antient dates there referred to, that is, he placed the capture of Troy at a full century later than the Argonautic expedition, and not at the 80 years after it of the *later* Greeks: the effect of which would be, that it was *possible* for him to have placed the commencement of Arbaces at 60 years before 1st Ol., but not that he *actually did* so; because we are still ignorant, whether that variation might not be compensated by some of his other sums being *less* than as I have stated them at the above-mentioned pages. Now the evidence for his having made a whole century of interval between the Argonautic expedition and the capture of Troy is to be found in his own Preface, where he says, "Græcos in ora Colchidis appulsos illinc rapuisse Medeam—*secunda* dehinc *ætate* [*γενεα*] Paridem Helenam rapuisse, &c." But what is the meaning of *γενεα* here? Certainly not a *generation* of 33 years, two *full* ones of which would amount to no more than 66 years at 3 to a century; and no antient ever made that interval less than 80 years, but many a

*istence* of the Median and Scriptural kingdoms in Assyria must certainly have greater weight than the merely *conjectural* opinion of Usher and his followers concerning the *later commencement* of Arbaces and the Medes than of the first Assyrian kings in Scripture : and as to the evidence of Ctesias, that the kingdom of Medes began on the

100 or 120 as Clemens informs us ; among which latter must have been Herodotus himself, for *γενεα* here must be used by him to mean a *century*, and thus *in the second γενεα after* would mean after 100 years. Agreeably to this sense of *γενεα* the Lexicon of Scapula says, it sometimes means *centum ann.* : but he gives no example of this sense from antient authors. However there is an example which occurs in the Septuagint in a prediction *that the Israelites should remain in Egypt until the 4th γενεα* ; now the Apostle says this was 400 years. Malela also says, “ that there were 5 *γενεαι* from Abraham to Moses, containing 443 years.” P. 74. He may have copied these words from a Greek writer of better authority. Yet Larcher translates *γενεα* in the above-mentioned Preface by *generation*, which is certainly erroneous : but Mr. Beloe more prudently by the ambiguous word *age*. When the sense in which an antient employs a word capable of two senses is no way determinable, it may be right thus to preserve the same ambiguity in a translation ; but this is not the case here, it being almost impossible, that Herodotus could here mean *generation* and not *century* : for no person is mentioned by him from whom a second generation is computable, and if there were, yet 2 full generations would amount to not more than 66 years.



the contrary *sooner* than the Scriptural kings of Assyria, the adherents to Usher cannot now with any consistency call his testimony back again as evidence against their *cotemporary* existence, after having rejected it when opposed to their own opinion of the still *later* existence of Arbaces as fixed by themselves, it being, they say, too fabulous to obtain any credit. But although it is not indeed fabulous, yet certainly his account was in part at its beginning *misled* by the fabulous period of 1460, to give an *earlier* existence to Arbaces, than the *historic* traditions of the Greeks had ascribed to him, and either Scriptural or profane evidence will confirm.\*

VOL. III. 3 D I have

\* There have been some persons indeed, who have gone still farther, and hypercritically denied any capture of Niniveh or dissolution of any kind to the Assyrian kingdom by Arbaces, but Mr. Falconer justly observes, "that it is too well attested [*by all profane authors among the antients*] to be overthrown by *conjecture*." P. 12. Others have pretended, that the account of the dissolution of that kingdom by Herodotus is in contradiction to the account of Ctesias. But this I have refuted in vol. ii. p. 398, and shewn, that there is no contradiction between them, and that the only article in doubt was to ascertain the duration of the *interval* between the conquest of Niniveh by Arbaces and the

I have now finished this *general view* of the *true historic* state of things during the

the separation of Media from the posterity of Arbaces by the usurpation there under Dejoces, which accordingly has been one of my objects in this enquiry. But I now find, that there is nothing new under the sun, and that truth has ever had its advocates, although they have not been regarded; for I find, that Cary had long ago contended for the same truths here as myself; he writes thus, "Usher has new modelled chronology by shortening the Median kingdom and making it commence *later* than 1st Ol.—If Scaliger had lived to hear of this adventure, what would he have said? He would have stood amazed to find, that all the memorials and traditions of the antients were nothing but romances; and more still that they erroneously ground such a mistake upon the words of Herodotus; although those words clearly prove, that the *first* revolt of the Medes mentioned there ended by a *freedom from tyranny*, by an *αυτονομία*, the later one under Dejoces reduced them under a new *tyranny* αὐτῆς ἐς τυραννίδας περὶνιδον. This *autonomy* was not an *anarchy* as they pretend, inconsistent with regal government, but only a government by their own laws, though under the authority of kings, just as with ourselves, by our not being subject to *foreign* power." *Cary's Account of Time*, p. 137 and 138. ann. 1677. This clearly vindicates Herodotus against differing from Ctesias in regard to there having been an enterprize by the Medes under Arbaces against the Assyrian kings *long before* the usurpation of Media by Dejoces, which separated it again from Assyria, after both those kingdoms had become subject to the Median posterity of Arbaces; it is only the *interval* which

Herodotus

the period of Median kingdom, so far as relates to the *cotemporary* existence of it with the Assyrian kingdom in Scripture, and also of the *erroneous historic* statements to the contrary which different moderns have invented and ascribed to that age and those kingdoms, by which they have either raised up the commencement of the kingdom of Medes *too high* to be cotemporary with the Assyrian kingdom or else brought it down *too late*, and in both cases urged by the same motive of giving an account how far the Scriptural history of the Assyrian kings differed from that of the Medes in profane authors; but which on the contrary when they are thus proved by chronologic dates still extant, to have been actually *cotemporary* (as Eusebius and Syncellus had rightly made them long ago, and doubtless from the evidence of some profane history now perished) the next step will be to shew how the same chronologic dates tend to prove still farther, that those kings thus found to have been cotemporary, were likewise in fact identically the *same persons*. The Jews indeed had first set an example of error, which

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some

Herodotus has left undetermined, and which Ctesias has determined erroneously.



some moderns have followed, in destroying the *cotemporary existence* of the above two royal successions; but it was through very different motives from the moderns, not from any conjectural and invented plan of their own to account for the apparent disagreement between their Scriptures and profane authors, but merely from an error concerning their own history in *later* times. For they erroneously conceived, that their 70 years of captivity ended at the accession of Cyrus to Persia, instead of his conquest of Babylon 20 years *later*; this raised up all their prior kings and events 20 years *too soon*; consequently along with these they equally raised up the reigns of the cotemporary Assyrian kings in Scripture. To this they added another error, for they placed the accession of Cyrus to Persia 9 or 10 years too soon as is done by Josephus also, which increased the error to 30 years *too soon*. The first Christians adopted from the Jews the chief part if not the whole of this error, and if any of them corrected any part of it, yet they introduced a third error of their own; for misled by some copies of the septuagint then current, they assigned to Amon 12 years of reign instead of the 2 years only in the Hebrew text, as did Eusebius: be-  
side.

side this they placed the destruction of the temple 4 or 5 years *too soon* before Cyrus as does Eusebius again, (although he has corrected some of the other errors) he making it precede Cyrus 30 years, when it was but 25 bef. Ol. 55. 1. These errors all tend the same way to raise up the Jewish kings and at the same time the Assyrian kings into a *too early* age to be *cotemporary* with the first kings of Medes according to the true *historic* date of their existence at 40 years or more before 1st Olympiad : and as to the *Asiatic* date of their commencement in Ctesias and Polyhistor, this was so *much sooner* even than the too early dates of Assyrian kings by the Jews, that the kings of the Medes were thus made to live *before* any of the first Assyrian kings in Scripture ; thus every way by such errors of one party or other the truth of their *cotemporary existence* was hid from the view of readers. What errors the Jews and first Christians thus committed through accidental mistake ought therefore to be distinguished from such similar errors caused by the intentional and preconcerted plans of the moderns ; although indeed it happened that the effect was the same, of destroying the cotemporary existence of the Scriptural and Median successions of  
 kings

kings in Assyria. These consequently form a *seventh class* of errors different from all those above-mentioned; and so far as can be collected out of Syncellus and the old Latin chronology in Scaliger, Africanus hence placed the Jewish kings 36 years too soon through the operation of the above errors, yet the accession of Arbaces nearly right: which becomes another proof that all three must have had some profane authority for their date of that event placed by them so much *later* than in Ctesias, and yet so consonant to one another, although without any apparent imitation one of another. Eusebius reduced the above error of Africanus to about 12 years *too soon*, and the commencement of the Medes he also corrected so as to be nearly right; but Syncellus made both successions to commence right and both together; in which he was followed by the *more early* modern writers, until Petavius, Usher, Newton and Jackson first disturbed that accession of Median kings by those antients; since which time there has been nothing but lawless conjectures and greater historic errors. This renders it the more necessary for all such as wish to form their judgment from actual evidence, and not be led about blindfold by the authority of others,



others, to attend to the information arising from my *seven* different antient testimonies, which I now proceed to state and examine in a more *particular* manner; and which will be all found to agree together in the principal articles requisite to a conviction of the *cotemporary* existence of the kings in question, and still farther of their *identity* also respectively: unless different persons can possess the very same kingdoms at the very same time, and also both ascend the throne and quit the world in the same years; and moreover two different kingdoms during 256 years, both begin and be destroyed by mere accident in the very same years. Before these coincidences by accident will happen a second time chaos will come again.

But previously it may be proper to point out the nature, utility and imperfections of the annexed table of reigns and dates. It is then only a different application of the plan adopted by Eusebius; for as it was his object by means of collateral columns to shew what reigns and events were cotemporary in *different kingdoms*; so it is my object by a similar method to shew what reigns and events are made cotemporary by different antient authors in *the same kingdoms*, and when  
not

not made cotemporary, how far they disagree with one another: but as it was begun solely for my own use to assist my memory and save the trouble of having recourse on every occasion to each of those seven authors; hence it was begun upon too contracted a plan by my not making a sufficient number of horizontal lines, so that no two reigns or events might be placed on the same line, if they did not take place precisely at the same date. But through the insufficient number of horizontal lines I could not always observe this rule, which however caused no obstruction to myself, as I knew my own meaning; yet it may cause perplexity to others: however I thought it better to publish it with this and some other imperfections, than not to publish it at all, as it required too much time and attention to improve the table; for notwithstanding any such defects it is still sufficient for the main purpose proposed by it, of shewing how nearly those seven authorities agree together with respect to the dates of the *same events in the same kingdoms*. This cannot so clearly appear in reading those authors themselves, because they often employ different modes of computation, sometimes by Olympiads, sometimes by the

the æra of Nabonassar, sometimes by only sums of reigns added together and reckoned either downward from some prior epoch or else upward from some later one, all which must be first reduced to one and the same standard of computing time, before readers can discern, whether they place the same events at the same dates or not, and how far they differ. Now this difficulty is removed at once by inspection of the table, where each horizontal line is intended to express one and the same date, though by different modes of computing time employed by different writers. This comparative table of events and dates may however be improved in a subsequent edition by myself or others by introducing more horizontal lines, if not also some perpendicular ones, so that every event, and the same events also in different authors, may have their own appropriate lines and dates distinct from one another: the effect of which will be, that readers will be surprised to find that those authors differ so little from one another, as that they rather harmoniously form an united body of historic evidence in direct opposition to the modern conjectural *makers* of antient history and chronology, from Petavius and Usher down to Falconer, the latest of this inventive school for the connection of pro-



fane and scriptural history by mere conjecture; and is all this positive evidence to be set aside out of complaisance to the fanciful *opinions* of some modern and favorite system makers?

No. 1.—*Polyhistor.*

The abridgement of Ctesias by Diodorus together with Justin have been hitherto the only antient authors, to whom the moderns have had recourse, in order to fix the *commencement* of the kingdom of Medes according to that more *early Asiatic* date of it at about a century before the Olympiads, more or less; but both those accounts are subject to such doubts and ambiguities, that the moderns nevertheless have not been able to collect from them the date of that event with any certainty and precision, and have consequently differed greatly from one another. So also Eusebius and Syncellus have been the only antients, who have been appealed to for the *later historic* date of 40 years more or less for the *commencement* of that kingdom before 1st Olympiad: and to which of the above two contradictory dates most credit is due no person has been hitherto able to determine. I have therefore here for the  
first

first time introduced two new authorities to public notice, Herodotus and Polyhistor, the former of whom adds confirmation to the *latest* of the above two dates, and the latter to the more *early* one: both of these testimonies have been hitherto either altogether unnoticed or neglected or misrepresented, although in reality that of Polyhistor is the most precise of any extant, and that of Herodotus the only *profane* evidence extant, which confirms the Christian ones of Eusebius and Syncellus; whereby it becomes very important toward proving, that the *later* date of these Christian chronologers was copied by them from some profane system of chronology current in their ages, and was not the invention of themselves for any purpose relative to a better accommodation of Scripture history to profane events. The testimony of Herodotus however receives some farther *profane confirmation* from the Assyrian period as mutilated by the later Greeks; since it appears, that they reckoned 365 years from the capture of Troy to the end of the Assyrian kingdom, therefore could not reckon above 40 more from thence to the 1st Olympiad in order to complete their total sum of 405 years. Moreover both Eusebius and Syncellus

every where write as if those, who thus mutilated the Assyrian period, placed the capture of Troy under Tautanes, and the old Latin chronologer in Scaliger confirms the same, adding in the 33d of Tautanes; this again rendered it impossible for those, who thus omitted the 4 Assyrian reigns of 162 years, to reckon more than 40 years for the interval from the end of the Assyrian kingdom to 1st Ol. That old Latin chronologer frequently copied after Africanus, for he places the beginning and end of the 70 years of captivity precisely in the *peculiar* method of Africanus, which was different from that of Eusebius and every other known method: now he again confirms the same interval of 40 years, for he says, that it amounted to 53 years down to the 1st Ol.; but Africanus had not improbably placed the 1st Ol. 10 years too late viz. on 11th of Æschylus instead of the 2d, just as Syncellus does, who was fond of following Africanus, and hereby himself made 54 years instead of 44 with Eusebius. Syncellus indeed is incoherent in this article, for although his sums by the years of the world place the 1st Ol. in the 11th of Æschylus at Athens, yet in his annexed text he says, it fell on the 2d. All these other accounts then,



then, the only ones preserved, concur in agreeing with Herodotus and Scripture to fix the true *historic* commencement of the kingdoms of Medes at 40 or more years before 1st Ol.; and thus they become important chronologic relics of the *profane* computations then current concerning the real *historic* interval in question, as it had been fixed by all those antients, who wrote history instead of the fabulous accounts of *Asiatic* chronology. From some profane chronology therefore of those times it must have been, that Eusebius and Syncellus derived their *later* historic date of the same event. But on the other hand Polyhistor must have copied his *earlier* date from the Asiatic account of Berosus; for Syncellus repeatedly informs us that the Assyrian accounts in the *historic collections* of Polyhistor were copied from that Asiatic historian: and what his date of this event was we have now to examine *more particularly*, that it may assist us to interpret rightly the account of Ctesias; who had before given a relation of the same Asiatic computations, although too dubiously expressed by Diodorus to be well intelligible. Justin apparently followed the same authority of Ctesias in Diodorus, but through some error made the total sum  
different

different from the amount as reckoned up by *any* of the moderns; in what his error consisted I shall be able to point out with some degree of probability and whence it arose.

It is Agathias, who has fortunately preserved the computations of these times by Polyhistor, his information concerning which I here subjoin from my vol. i. p. 341, where I have produced it before\*, but which had been never hitherto attended to by any modern author. Usher indeed appears to have known of it, but neglected it.† Vignoles is the only modern, who has quoted a part of it only, and in so doing misunderstood it, and consequently again rejected it as being *too vague* and useless; although it is in reality the only one extant, which enables us to fix that *more early* date to a single year: all others, beside several doubts attending them,

\* “Cum Medi annis *non minus trecentis* imperio potiti sunt, Cyrus Cambysæ filius, Astyage debellato, ad Persas imperium transtulit—Alexander autem Perside devicta ad Macedonicam gubernationem res transtulit—quum vero imperassent Macedones *non minore spatio* quam Medi, *septem* videlicet annis minus, *Romanis* fracti sunt, nam etiam hac in re credendum est *Polyhistori*.” Lib. 2.

† “Agathias in lib. 2. de Bione et Polyhistori.” Usher ap. ann. 748.

them, yet at best only approximate nearly to the intended date in a general manner. For Ctesias and Justin not only differ, but different moderns disagree also so much concerning the total sum arising from the computations of Ctesias himself, as to make a difference of 50 years; whereas the account of Polyhistor fixes the event exactly to the 307th year before the accession of Cyrus, which was the 91st before 1st Olympiad, and the 120th before Nabonassar.\* In this year was the end of one period

\* According to Vignoles " Agathias says, as if after Ctesias, *that the Medes having obtained the empire, kept it not less than 300 years* [στην ου μείον η τριακοντα], but no author has made so many as 350 except Justin; this latter has the only passage of the antients, which has fixed the date precisely; Ctesias or rather Diodorus have not done the same: as to Agathias, he contented himself with saying, *that the Medes did not reign less than 300 years*, which is *too vague*." Vol. ii. p. 234 and 243. Now here Vignoles supposes the words of Agathias to be copied from Ctesias, yet Agathias himself expressly refers them to Polyhistor. Moreover, he only quotes the first part of the sentence, which by itself may indeed be *too vague*; but it would have been otherwise, if he had attended to the remainder of the sentence, which limits and explains the vague nature of the first words even to a single year. I have already mentioned this in vol. i., and although the account is indeed expressed in a cramp and circuitous way, yet it results from the whole, that he must



period of 1460 and beginning of another,  
but such a coincidence of the end of the  
Assyrian

must have found Polyhistor to make exactly 7 years more of the kingdom of Medes than of the Macedonians, not 7 years less. Now the Macedonians ruled full 300 years, therefore the Medes 307. This result appears hence, for he says *that the Macedonian dominion had lasted not less than that of the Medes, except only by 7 years less, when it was destroyed by the Romans*; but the Macedonian dominion lasted exactly 300 years from the victory at Arbela to the death of Cleopatra, at the conquest of that last Macedonian kingdom in Egypt by Augustus, which put an end to all Macedonian power: and he must mean this end of the Macedonian kingdom in Egypt, because he mentions, *that it was destroyed by the Romans*; for the Macedonian kingdom in Syria had been destroyed 52 years before, and not by the Romans, but by the king of Armenia. We find moreover other examples, of the antients reckoning down the existence of Macedonian power until the death of Cleopatra, and that they made it amount to the same sum of 300 years. Thus when Jerom states the duration of different kingdoms he allows 300 to that of the Macedonians, he must therefore have reckoned down to the above conquest of Egypt by Augustus. "*Ipsi Macedones regnaverunt annis trecentis.*" *Comm. Daniel c. 9.* Africanus also in a passage quoted from him in Syncellus at p. 308 reckons the whole Macedonian dominion at 293 years down to the death of Cleopatra; by his making it amount to 2 less than 300, he must have reckoned it only from the death of Darius 2 years after the victory at Arbela. "*Macedonii imperii series stetit post Persarum regnum annos 298: omne igitur tempus* a Ma-

Assyrian kingdom after 1460 years with the end of an astronomic and astrologic period of 1460 could have scarcely happened in reality from the accidental course of human affairs; it rather betrays therefore scientific art being employed here in forcing the history of Asia into a conformity with the visions of astrology, agreeably to the established practice, as I have shewn, in more antient ages there and still found subsisting among oriental nations. Such an improbable extent of duration for the earliest kingdom on record among mankind renders it still farther subject to suspicion of scientific fraud; and still more so, by our having historic testimonies now extant in Herodotus, Eusebius, Syncellus, Orosius, Africanus and Jerome, that the event was not believed to have actually taken place until 50 years later. These circumstances then shew us, not only that this date by Polyhistor of 91 years before 1st Ol., was connected with the artificial chronology of Asia, but also the reason,

VOL. III.

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a Macedonum initio ad Cleopatram et Egyptiaci regni finem includitur." Again at p. 261 Syncellus himself reckons up the same sum of 300. "Ab Alexandri anno 7 ad 22 Cleopatrarum sunt anni 300." But he says the 7th year instead of the 5th, or else he should have made only 298.

why the event was fixed at that particular year; facts which we could not have easily discovered without this very date itself: it gives us therefore the more confidence to conclude, that the date *intended* by Ctesias was the very same as in Polyhistor; and that if the sums of Ctesias, as ambiguously enumerated by Diodorus, are capable of being reckoned up in such a manner as to fix the 1st of them on the 91st before 1st Ol., just as well as on any other year, this ought to have the preference: more especially also, if by this mode of reckoning them up, they will be made consistent with the accounts of other profane historians *in other articles* instead of being in contradiction to them. So that it will operate as an interpreter of the original *meaning* of Ctesias, which Diodorus by his brevity has not clearly expressed; and also as an arbitrator between the different modes of computing the sums of Ctesias contended for by different moderns, on which they have founded different and even opposite plans of Asiatic history. Hence it will be found, that the 3 antients, who had adopted the *Asiatic* chronology have all adopted also the very same 91st year for the *commencement* of the kingdom of Medes: and also that the  
other



other antients, who have adopted the *Greek* chronology of that kingdom, have also all agreed within 10 or 20 years in the same date of about 40 years or more before 1st Ol. Thus no conjectural alteration is made in the text of the MSS of Diodorus or of others, in order to force otherwise discordant sums into an agreement with the accounts of other antients; which is a very arbitrary and objectionable method, although too much countenanced in the present times: but if we can select out of different readings in those MSS and out of different senses of which the words of Diodorus are equally capable, such readings and such senses as shall produce harmony on all sides, this is certainly a reasonable mode of interpretation; and this we shall be able to effect by means of the date in question afforded by Polyhistor, which Vignoles rejected as useless, and all other moderns have omitted so much as to mention. The want of such an arbitrating evidence as this has been the cause, that the moderns have embraced such different opinions; and that readers have been so long distressed by their discordant accounts of this period of history, for they were not able to find any evidence which might induce them to prefer one account before

any other : but now that useful office of an arbitrator, which Polyhistor discharges with respect to the *early* Asiatic date of 91 years, Herodotus will perform the same acceptable office in regard to the *later* Greek date of 40 years before 1st Ol., for the commencement of the *historic* kingdom of Medes. He does not indeed mention such circumstances, as may enable us to fix the *historic* date of that event with so much precision as Polyhistor does the *astronomical* date ; yet as it appears to be somewhere between 40 and 60 years before 1st Ol., this is sufficiently near for our purpose of proving that *Arbaces* the first king of the Medes in profane authors lived in the *same times* as *Jarib* the first king of Assyria mentioned in the Jewish Scriptures : and in fact neither are we able to fix more exactly the commencement of *Jarib*, he being mentioned only by Hosea, and only as having been king of Assyria *not long before* he prophesied in the reign of Jereboam 2d. This, as B. Horseley thinks, must have been late in that reign, because he is said in the title to have also prophesied at the beginning of Hezekiah ; now there were 68 years, he says, from the death of Jereboam to the 1st of Hezekiah. But there is some difficulty to the amount

amount of 10 or even 20 years in fixing also the death of Jereboam; however, if we allow those 68 years to be right, yet still the 40th before 1st Ol., would be 92 years before Hezekiah, from which deducting 68, the remainder would be 24 years before the *death* of Jereboam for the commencement of Arbaces or Jarib, which was the 17th of Jereboam and 10 years before the accession of Uzziah on his 27th: somewhere then in those first 17 years of Jereboam *Arbaces* appears to have begun, and *Jarib* also to have then reigned, possibly at about 20 years *before* Hosea prophesied at the *end* of Jereboam's reign. Accordingly Syncellus fixes Arbaces on the 6th of Jereboam, and Eusebius on the 19th; but if we correct his error of giving 10 years too many to Amon, the accession of Arbaces would then fall on the 9th of Jereboam: and there *might* Jarib have begun likewise, so far as we can discover from Hosea; and thus Eusebius and Syncellus would differ only 3 years in fixing the accession of Arbaces and the commencement of the kingdom of Medes. From these dates then it appears, that Arbaces and Jarib must have lived in the *same times* and *might have* commenced even at the very same year, so far as we can obtain  
any



any knowledge of the actual years of the commencement of *either* of them; for which purpose therefore we thus find, that the information derived from Herodotus as to their *historic* beginning is quite *sufficient*, although indeed not so *precise* as that of Polyhistor for their *astrologic* beginning. For if instead of 40, the date intended by him should have been 60 before 1st Ol., yet this would still only place his accession a few years *before* the accession of Jereboam 2d instead of a few years *after* it; and it is not ascertainable from Hosea, whether the application of the Jews to Jarib king of Assyria for his assistance against the Syrians was made in the *beginning* of Jereboam's reign or near the *end* of the reign of his predecessor Joas. But whatever the real fact was with respect to the time and date of that application, yet any way the *cotemporary existence* of Jarib in Hosea with that of Arbaces in Herodotus, Eusebius and Syncellus, Orosius and Jerome at about 40 years before 1st Ol., still becomes evident; the accession of Jereboam being about 50 years before 1st Ol.: while at the same time the dates of the commencement of Arbaces by all these authors differs very considerably from the 91 years of Polyhistor and Ctesias before

before 1st Ol.; so that the former authors must have had some profane and Greek *historic* account before them, although not transmitted to us, for this common foundation of their same opinion, and as the means of having brought them to such a near agreement with one another in their dates of that event. Just as I have shewn, that in like manner Berosus, Polyhistor, Ctesias and Justin, had also on their side a common foundation, through Asiatic information, for the near agreement of their dates also of the same event at half a century *earlier* than that of Herodotus, Eusebius, &c.

It only still remains, that I give some explication of the several dates inserted in this 1st No. relative to Polyhistor for the better comprehension of readers. Now the first line mentions the date before Christ 867, at which the accession of Arbaces is fixed by Polyhistor; from which date down to the 1st Ol., in 776 before Christ were 91 years, as is there marked in the first perpendicular column; and from that down to the first year of the æra of Nabonassar were 29 years more : whereby the dissolution of the Assyrian kingdom by Arbaces is placed at 120 years before the epoch of Nabonassar, as is marked also in the 2d perpendicular

perpendicular column, containing the number of years as *collected* from the earliest date down to the later ones, therefore 120 years from Arbaces to the æra of Nabonassar. From the 1st of which æra down to the 4th year of 54th Olympiad inclusively were 187 years more, which added to the 120 before Nabonassar make up the total of 307 years before Cyrus, as the date of his accession was generally fixed by the antients, viz. on Ol. 55.1; but thus 3 years more are to be still reckoned down to Ol. 55.3, inclusively, the 190th of Nabonassar, which I have proved to be the true date of the accession of Cyrus to Persia: hence 310 becomes the real sum for the total from the accession of Arbaces to the *true* accession of Cyrus; but this is only according to the computation of the arbitrary Asiatic period made known to the Greeks by Ctesias and Berosus, and copied from them by Polyhistor; which total is accordingly noticed by the lowest sum 310 in the second perpendicular column, which contains the *collected years* from the first date to the later ones.—It must be observed likewise, that the 1st year of Assaradinus in the æra of Nabonassar (which I have made cotemporary with the 2d year of Dejoes the usurper of Media as he is fixed by



by the sums in Herodotus) forms another epoch of computation in my table on account of its being capable of being fixed with more accuracy than any of the preceding events; for which reason it is separated from those preceding events and reigns by short lines as thus — — — — —

in order that it may be the better noticed by readers, and that they may always keep in mind, that the dates of events in the lower portion of the table are fixed with more certainty than in the upper portion of it; and this also by Ctesias himself, whose *Artæus* is the same person as that *Assaradinus* of the æra of Nabonassar. Thus the two dates of the *accession* and *death* of Assaradinus in the *middle* of our table (which we are fortunately enabled to fix with certainty from Scripture, Ctesias and the æra of Nabonassar) may be considered as secure and unerring *guide-posts* in the *middle* of our journey, whether we move upwards or downwards through this almost unknown, and hitherto undescribed country in the memoirs of antiquity. So that we have hereby obtained and noticed in the table three principal and well-ascertained dates in this period from the commencement of Arbaces to that of Cyrus, namely at the *beginning*, the *middle*, and the *end* of it :

and the evidence, which we shall afterwards discover for the identity of the Artæus of Ctesias with the Assaradinus of the æra of Nabonassar, the Esarchaddon of Scripture, will contribute still more to confirm the accuracy of the above *three* principal dates in the table.

*No 2. and 3.—Æra of Nabonassar and Ctesias.*

The contents of No. 2. and 3. are so interwoven together, that they must be taken into consideration in the same section, for we shall want repeatedly to compare the events and dates in the one with those in the other, in order to point out their connexion, consistency and harmony. Now No. 2. in my table contains the dates of the *accessions* and *deaths* of only *three* of those 10 kings of the Medes enumerated by Ctesias in No. 3, namely of those last 3, who through the conquest of Babylon by Esarchaddon the 6th of them became kings of Babylon also; they being before kings of Assyria only, although *Median descendants* from Arbaces, Media having just before the accession of Assaradinus been separated from his dominion by the usurpa-  
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tion there under Dejoces: these 3 kings were by the Babylonians in their æra of Nabonassar called Assaradinus, Saosduchinus, and Chyniladinus, which last, so far as we can judge, was the king, who was deposed and slain by Cyaxares, grandson of Dejoces, at his conquest of Niniveh and total dissolution of the kingdom of Assyria. These three then in the æra of Nabonassar must be the *same persons* as the three last kings in Ctesias of the Median posterity of Arbaces, namely the 6th, 7th and 8th, called by him and the Persians Artæus, Artynes and Artibarnas, all three evidently Persian names; and the language of Persia by rather approaching to the Gothic tongue of Scythia than to the Chaldee language spoken at Babylon and in Syria, was the cause of this difference in appellation to the same persons by two nations, one living on the east and the other on the west of Babylon and speaking two very different languages: on this account the Jews and Syrians on the west, who spoke dialects of the Babylonian tongue, did preserve in some measure those proper Babylonian names, *Assaradinus* being called in the Hebrew of the Jews *Esarchaddon*; but what they called his two successors we are ignorant, they being not named in the



Jewish Scriptures although occasionally spoken of there. From the Syrians on the west of Babylon the Greeks also had derived some knowledge of the Babylonian names; *Senacherib* and *Esarchaddon* having been corrupted by them into *Sancheribus* as in Herodotus, and *Asordan* or *Sarchedonus* in the septuagint Greek. It is no way strange then, that all the Median posterity of Arbaces in Ctesias should have other names among the Persians (from whom alone Ctesias obtained his account of them) than those by which they were known to the Jews Syrians and Greeks: and this we find confirmed by Ctesias who expressly says, that of the two last of those kings of Media *Astibara* and *Apandas* the 9th and 10th, the latter was the same person as was called by Herodotus and the Greeks *Astyages*.\* But he might have said the same of the predecessor *Astibara*; since it is noticed there, that he was the *father* to *Apandas*, and we know from Herodotus, that the father of *Astyages* was called by the Greeks *Cyaxares*; so that here again we find *Cyaxares* the Greek name for this *father* to be quite different from *Astibara* the

\* “ *Astibara* Medorum rege extincto filius ejus *Apandus* regnum suscepit, quem Græci *Astyagem* vocant.” *Lib. 2.*

the Persian name of him, just as was the case with his *son* : Polyhistor accordingly in a sentence quoted from him by Eusebius expressly says “ that Astibara was called by the Greeks Cyaxares” [*see my vol. ii. p. 180, and Euseb. præp. Evang. 9. 39.*] Since then we here find with certainty the last two kings in the catalogue of Ctesias to have quite different names among the Greeks from those in the Ctesian catalogue, how can we hesitate to allow, that the case *may have* been the very same in all the preceding 8 kings as in these two last of them the 9th and 10th ; and in the same manner that the Jews also and Syrians may have known all those 8 first kings in Ctesias by totally different names from what they were called by the Persians in his catalogue. Even among the Greeks and Romans themselves we sometimes find confusion arising from a similar contrariety of names for the same person, or else of the same name for different persons ; thus Antiochus one of the Syrian kings, who was called *Grypus* by some nations, is by Josephus called *Philometor*, while on his own coins he stiles himself *Epiphanes*. So in Egypt also Ptolemy commonly called *Lathyrus* stiles himself *Soter*, while some antient authors call him *Philometor*. Even among the  
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Romans great confusion has arisen from several emperors of very different original names assuming the same name of Antoninus; and the same happened also in Persia by kings originally of very different names assuming the names of Xerxes and Darius; who have therefore been sometimes by antient authors called by these and sometimes by their original names: nay in Scripture itself we find different kings of Persia called by the same name of *Artaschesha*, and also the same king Cyrus called *Coresh* in Isaiah and *Darius* in Daniel; nay in Daniel apparently both *Coresh* and *Darius*; and Salmanassar is in Tobit called Enemessar. But it was not only by different nations that the same kings had different appellations given to them, for the same person often himself assumed different names at different periods of his reign according to the new events which happened to him, and as often took up new names on his birth-day as a new suit of cloaths; which custom of antient oriental nations even still subsists, for we know that *Tippoo Saib* assumed a different name in the last years of his life. Hence it is evident, that *names* alone are not a sufficient designation of *persons*, and it is in vain to object to the *identity* of the Ctesian kings of the Medes with the Assyrian



syrian kings of Scripture on account of their having different *names*; we ought rather to depend for identity of *person* upon proofs of the identity of *times* of existence, *countries* possessed and *actions* performed: but in the present case, as through the loss of all antient histories of these ages, we unfortunately know but little of the *actions* performed, we must be content therefore to set the greater value and place the more dependance upon what has been preserved, the *dates* of their existence and the *countries* possessed by the kings in question. Since then we have certain information from Ctesias himself and Polyhistor, that the two last kings of the Medes in Ctesias the 9th and 10th were the *same persons* as the two last kings of *Media* in Herodotus, although both of them very differently named, we may not unreasonably *presume*, that their three predecessors in Ctesias, *Artæus*, *Artynes* and *Artibarnas*, the three last kings of the Medes of the posterity of Arbaces before Cyaxares or Astibara, might be the *same persons*, as the three last kings in the æra of Nabonassar before the capture of Niniveh by that same Cyaxares. But this probable *presumption* will be confirmed beyond all doubt by the identity of the *dates of their existence*, for Artæus both be-  
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*gan* and *ended* his reign at the very same dates as Assaradinus; and his two successors Artynes and Artibarnas nearly if not exactly at the same dates as Saosduchinus and Chiniladinus: the reign of Artibarnas moreover *ended* at the very same date, at which Eusebius has fixed the capture of Niniveh and the *end* of the Assyrian kingdom by the conquest of it under Cyaxares *i. e.* Astibara, who is likewise made in Ctesias to be the immediate successor to Artibarnas in his catalogue of kings of the Medes. Now we have no knowledge, but that as Chyniladinus was the last king of Babylon in the æra of Nabonassar of the *Median* race of Assaradinus, and certainly *began* there in the very same year as the Artibarnas of Ctesias; so he *might* also, so far as we can know, have prolonged his reign, without having any successor until the *end* of the Assyrian kingdom effected by Astibara the Greek Cyaxares: hereby his reign would amount to 40 years from the date of his accession on the 101st in the æra of Nabonassar down to the 140th, which is *one* date of the capture of Niniveh in Eusebius. But 40 is the very duration, which Ctesias assigns to Artibarnas, who therefore apparently was the same *person* as Chyniladinus; the date of whose *death* could not in  
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course be found in the æra of Nabonassar, because he did not reign at Babylon until his death, being expelled from that throne at the end of the year 122 by the usurper Nabopollasar; but there is no cause to conclude, that therefore he died in that year because he then no longer reigned at Babylon: for he might still have continued his reign in Assyria, and probably did so, until the dissolution of that kingdom by Cyaxares; since Ctesias allows that very sum of 40 years to the *last* Median king descended from Arbaces *before* Astibara, who conquered Niniveh and Assyria. These identities of the *accessions* and *deaths* of the three last kings of Babylon in the æra of Nabonassar with the *accessions* and *deaths* as dated by Ctesias of the three last kings of the Medes are so important, that it is rather wonderful, they have never hitherto been noticed by any modern author: of much more importance certainly such identities of dates concerning *reigns* must be, than the identities of *names* only, of which the same are assumeable by quite different persons in very different countries, and in different ages; whereas those of *accessions* and *deaths* and the consequent duration of *reigns* depend solely upon the natural course of human affairs



under God's providence, not the uncertain controul of man. But nature is scarcely ever so uniform in its operations concerning man, as to raise up exactly the same events of existence and death to very different kings in exactly the same spaces of time and in countries contiguous to one another: this indeed may be within the bounds of *possibility*, when those reigns happen in different kingdoms; but it becomes an absolute *impossibility*, if they happen in the *same kingdom* and at the *very same time* and in the *same manner*, unless those kings, though called by different names, be nevertheless the very same *persons*. The voluntary caprices of human will, by which alone names are affixed, follow no uniform rules whatever, but such an uniformity of facts as above-mentioned in the operations of nature becomes an indelible mark impressed by nature itself, and ever after necessarily accompanying such kings, to testify that they could not be two but only one and the same person, though known by different names to different nations.\* That the *Sancheribus* of Herodotus

\* Agreeably to this M. Michel referred to above at p. 342 says, " If any one should object against the identity of the Scriptural kings of Assyria

Herodotus was the same person as the Sennacherib of Scripture was never doubted by any one; nor that the *Assaradinus* of the æra of Nabonassar was the same as the Esarchaddon of Scripture; and if his two successors had been mentioned in Scripture we should possibly have found them to have some similitude to the names in that æra, but the Scriptural narration ends with Esarchaddon. However in Judith and Tobit mention is made of some *later* king of Assyria, yet his current name is not introduced; this however must have been in both these cases the Chyniladinus of that æra, the last Median king over Assyria before its dissolution by Cyaxares, as the dates of those narrations sufficiently prove.

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syria with those of the Medes in Ctesias, that they have very different names, it should be observed, that the same kings were known to the Persians by different names from those current among the Jews and Greeks." P. 235. Jackson also says, "The Greeks called the same king Astyages, whom Ctesias called Aspandas; the reason of these different names is, that one is the name he had before he came to the throne, and the other what he then assumed; on the same account the Assyrian, Babylonian, Persian and Egyptian kings had two names, and by historians are called either by one or the other." P. 254. This may be one cause, which is assigned by Jackson, but there were a variety of others.

Whether at the beginning of the same kingdom there be any traces of the Greek name Arbaces in the Scriptural *Jarib*, others may determine, as they please ; but they certainly were *cotemporary*. So that at both the *beginning* and *end* of that kingdom we have sufficient chronologic evidence from *dates* to become a means to correct the dissimilitude of names and titles and the deficiency of historic information ; and thus to prove, that those *three last* kings in the æra of Nabonassar, before the usurpation at Babylon by Nabopollasar, Asaradinus, Saosduchinus and Chyniladinus were the *same persons* as the *three last* kings of the Medes in Assyria enumerated in the catalogue of Ctesias, Artæus, Artynes and Artibarnas : they reigning in the former case 40, 20 and 42 years, but in the latter 40, 22 and 40, i. e. 102 in all from the death of Sennacherib to the capture of Niniveh by Cyaxares, and this final dissolution of the kingdom of Assyria under the Median posterity of Arbaces ; who were therefore by Ctesias called kings of the Medes, like their predecessors before Media was usurped from them by Dejoces, but in Scripture are called kings of Assyria, over which country they continued still to reign, until the final destruction of their



their dominion by Cyaxares in the 140th, 141st, or 142d year of Nabonassar, as may be seen in my table. Those 3 kings were consequently succeeded by *Astibara* who conquered the last of them, i. e. by Cyaxares, and at his death his son *Apandas* or *Astyages* succeeded to both his thrones over Assyria as well as Media; both which kings therefore are rightly enumerated by Ctesias in his catalogue among the kings of Assyria, although they were through the above conquest of a different family from their predecessors in Assyria, the posterity of Arbaces. But Diodorus has omitted here the durations of the reigns of these two last kings, and probably because he had mentioned them afterwards in their proper places in one of his books, which has perished, where he must have related the usurpation of Media by Dejoces and the history of his three successors ending with the conquest of the last of them *Astyages* by Cyrus.

The *dates* then of the above-mentioned three kings in the unerring æra of Nabonassar, which agree so exactly to the dates of the same three kings in the catalogue of Ctesias form a conspicuous bond of connexion and union between all my seven authorities in the table; but more particularly

larly the date of the *death* of the first of them Assaradinus in the 80th year of that æra ; I have therefore considered the date of his *accession* in 141 as a new epoch of computation, for although it is not quite so certain as his *death*, yet it can be very nearly ascertained from Scripture, and by its commencement at the death of Sennacherib, thus fixes the *time* of existence of all the preceding 5 Assyrian kings in Scripture as well as in the catalogue of Ctesias : hence it becomes a secure *foundation date* in the *middle* of the period of Median empire, and a check upon all my other 6 authorities ; which must consequently be adjudged to be exact or not according as they agree or differ in their dates from that. From this foundation date then of the death of Esarchaddon in the 80th year of the æra of Nabon, we can ascend both upwards to Arbaces in our more *particular* enquiries, and also downward toward Cyrus : and hence in the first place it follows, that Artæus the 8th Median king of Ctesias must be the *same person* as the above Esarchaddon. For we find in Ctesias, that he reigned 40 years, as did Esarchaddon from 40 to 80 ; and if we reckon *downward* from his death we find, that there are 62 years assigned by Ctesias  
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to his two successors Artynes and Artibarnas down to the dissolution of the Assyrian kingdom at the conquest of it by Astibara (Cyaxares): this is likewise the very sum of years intervening between that 80th year and the 142d year, which year either was part of the 1st of Jehoiakim or next to it, and was *cotemporary* with the 3d year of 43d Olympiad, *the very year* at which some of the MSS of Eusebius in No. 5 fix the end of the Assyrian kingdom at the capture of Niniveh by Cyaxares. But the MSS of Eusebius vary here a little, and some of them fix that event 2 or 3 years *sooner*, which however might perhaps agree rather better than otherwise with the æra of Nabonassar, as may be seen in my table. As these variations then in the MSS of Eusebius render it not quite certain whether there were 60, 61, or 62 years of interval from the death of Artæus to the capture of Niniveh, we may prove the fact of the *cotemporary* existence of Artæus and Esarchaddon with still greater accuracy by reckoning *upward* to Arbaces. For the sums of all the reigns from the accession of Arbaces in Ctesias to the death of Artæus are 200; so also in the æra of Nabonassar there were 200 years from the 120th year before Nabonassar, at which date Polyhistor  
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places the accession of Arbaces, down to the 80th year of that æra at which date Esarchaddon ended. Thus it appears, that the Artæus of Ctesias both *began* and *ended* his reign over Assyria according to Ctesias, in the very same years as Esarchaddon *began* and *ended* his reign over *the same kingdom* according to Scripture, and this confirmed by the æra of Nabonassar. How then is it possible to be otherwise, than that Artæus and Esarchaddon were one and the same *person*, since they were both kings of Assyria at the *very same time*? This will also follow still more evidently from what I shall prove afterwards, that Artæus reigned likewise over Babylon and Persia at the very same time, that Esarchaddon also reigned over these *same two kingdoms* as well as over Assyria: but it is impossible, that two *different persons* could reign over the same three kingdoms at the very same time.

Now as it was through the conquest of Babylon by Esarchaddon that the dates of his own death and also those of his successors came to be admitted into the æra of Nabonassar, consequently this æra can give no assistance *upwards* with respect to the preceding 5 kings over Assyria, either in Ctesias or Scripture; we will therefore  
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examine first of all the reigns *downwards* from Esarchaddon to Cyrus, that we may have the benefit of the æra of Nabonassar to assist in our enquiries: and still more especially because those three last kings over Assyria lived but a short time before the reign of Cyrus, and the first dawning there of authentic history; so that a few *historic* scraps concerning them have been preserved by other antient authors, to assist our *chronologic* enquiries. It was therefore on account of the accuracy of the dates of accessions and deaths of these *three* kings in the æra of Nabonassar, that I have arranged them in No. 2 previously to the catalogue of Ctesias in No. 3, containing *all* the 10 kings of the Medes; as we can hereby fix the latter with more certainty, so far up at least as to the 6th inclusively in the *middle* of that period of empire: beside which, these three last of them lived in times better known than their predecessors; so that herein I follow the same method as at first of proceeding from what is best known to what is more unknown; and still farther, the chief doubts and difficulties, which occur in summing up rightly the duration of the reigns of these 10 kings in the too brief abridgement of Ctesias by Diodorus, are to be found during

the reigns of these same three kings and their immediate successor Astibara. For all these reasons it is expedient to begin our more *particular* enquiries with these 3 kings in question, who immediately preceded the *end* of the kingdom of Medes.

We begin then with proposing to fix the date of the *accession* of Assaradinus to Assyria the first of the three, having already ascertained the date of his *death* by the æra of Nabonassar to have been along with the 80th year of that æra. Now it is agreed by all parties, that this king was the same person, who is called Esarchaddon in Scripture, and that he came to be enumerated in that æra on account of his having conquered and thus made himself king over Babylon; so that the date of his accession to Babylon is only the date of his conquest of it in the 68th year, and he had been king over Assyria many years before in consequence of the death of his father Senacherib: but this event in course had no concern with Babylon, therefore the date of it is not to be found in the æra of Nabonassar; yet from Scripture however we are enabled to discover it, and fortunately with more accuracy, than the dates there generally afford opportunity of doing. For it is expressly said, that Senacherib invaded



vaded Judæa in the 14th of Hezekiah, which as may be seen in my table vol. ii. 216. was the 37th of Nabonassar; from Judæa he proceeded into Egypt but returned hastily into Judæa, and thence back to Assyria, soon after which he was slain by two of his sons. Herodotus likewise mentions this invasion of Judæa by this king, whom he calls *Sancheribus*, but does not ascertain the date of it; the prophecy however of Isaiah concerning his quick return home proves, that Senacherib must have left Judæa on his return to Assyria early in the 3d year from his first arrival there in the 14th of Hezekiah. For the prophecy says, "In the 3d year sow ye and reap and eat the fruit thereof." 2 King. 18. If these 3 years are reckoned inclusively of the 14th, he must then have quitted Judæa at the beginning of the 16th of Hezekiah; but if exclusively on the 17th, which is the *latest* possible date, and this was the 40th of Nabonassar. Now what time intervened between his return from Judæa and his death is not certain, but the manner of its relation indicates, that it was *soon after*, for it is said, "So Senacherib king of Assyria returned and dwelt at Niniveh, and as he was worshipping in the house of Nizroch his god two of his sons slew him,

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and Esarchaddon his son reigned in his stead." This relation seems to imply, that one event followed soon after the other, yet it leaves room however for a longer interval: but the account of the same facts in Tobit still more strongly implies that the interval was very short, since the writer computes even by the number of *days* and those only 55, after he had buried a Jew slain by Senacherib soon after his return from Judæa. "If Senacherib had slain any [Jew] after he was come to Niniveh and fled from Judæa, for in his wrath he slew many, then I buried them, and all my goods were forcibly taken away, but there passed not five and fifty days before two of his sons killed him and fled, and Sarchedonius his son reigned in his stead." i. 18. Those 55 days may possibly be reckoned only from the time that his goods were taken away, yet this is not certain; but as the king's *wrath* against the Jews would be most violent at first, the events referred to must have happened immediately after his arrival at Niniveh: consequently if we place his death 1 full year later than his evacuation of Judæa at the *beginning* of the 40th year, therefore at the *end* of that 40th year, then the first of Esarchaddon would be the 41st year, though indeed possibly 1 year sooner  
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in case the *third* year is reckoned inclusively of the 14th of Hezekiah; but any way we cannot be far distant from the truth, since from the 40th exclusively to the 80th inclusively are 40 years, and this is the very sum of reign assigned to Artæus by Ctesias. This date is also confirmed farther by another sentence in Tobit, which indicates, that the usurpation of Media by Dejoces had *preceded* the return of Senacherib from Judæa; for in the 15th verse Tobit says, "that in the reign of Senacherib his estate was troubled, so that I could not go into Media." This *trouble* referred to was probably caused by the usurpation of Media by Dejoces; but the return of Senacherib to Niniveh is not mentioned until 3 verses *after* in the 18th. "And if king Senacherib had slain any, when he was come and fled from Judæa I buried them." Now the accession of Dejoces is fixed by Eusebius on the 1st of 18th Ol., which was the 40th year of Nabonassar, therefore the return and death of Senacherib must apparently have been *later* than that accession, which it would be, if fixed at the *end* of the 40th; so hence again the 41st was the 1st of Esarchaddon and the 40th before his own death. From these circumstances it follows, that his accession could not be *later*



*later* than with the 41st, but possibly might have been 1 year *sooner*, if *third* year is to be understood inclusively. Accordingly Riccioli does understand it inclusively, for he observes upon these words in *3tio anno seminate*, "i. e. quia propter incursiones Assyriorum non licebat serere—sed liberatum esse Hezekiam tertio anno, id est in 16 ann., percusso jam Senacherib." P. 232. Here we see also, that he conceived the death of Senacherib to have followed *immediately* after his return in the 16th year.\* Usher also and Patrick reckon inclusively and thus place his return on the 16th likewise, and his death in the same year; but by all these writers fixing the Jewish kings 2 or 3 years *too early*, they make the death of Senacherib *precede* the usurpation by Dejoces in Media; whereas it is not only probable in itself, that Dejoces took advantage of the absence of Senacherib to usurp Media, but there is in Tobit, as I have shewn, a direct indication that the troubles in Media and probable usurpation by Dejoces *preceded* the return and death of Senacherib: and this is confirmed by another fact related in Scripture, namely,

\* So also did Josephus, for he says, "Ad Ninum regiam rediit, ubi paulo post obiit." *Ant.* 10. 2.

ly, that Senacherib did not, because he could not return to Ecbatana, the former seat of government, but stopt short at Niniveh and *dwelt there*; Dejoces apparently being *then already* in possession of Media. This is the first mention in Scripture of any of the Assyrian kings residing at Niniveh, and Senacherib seems to have been the first who did so; not through any choice of removing the royal residence, but through necessity, because he had lost possession of Ecbatana and *could not* return thither.\* We see hence that by the Jewish reigns being placed by me 3 years *later*, as they ought to be for other reasons, then the *end* of the 40th year of Nabonassar, at which

\* It has been objected against Ctesias, that he says erroneously of Arbaces, that he removed the seat of government from Niniveh to Ecbatana, for it appears, it is added, both from Scripture and Herodotus, that it continued at Niniveh. To this it has been replied, that although Arbaces removed it to Ecbatana, yet some of his posterity afterwards might remove it back again to Niniveh; and some have said one king and some another, yet possibly none of them have fixed on the right person. For there is no mention in Scripture, whether the first Assyrian kings dwelt at Niniveh or Ecbatana, nor until this information concerning Senacherib; and that it should be here mentioned for the first time seems to have been done on account of the novelty of it, as being an act different from former practice.

which Senacherib died in 17th of Hezekiah, the 3d year exclusively from the 14th, will be *later* than the usurpation by Dejoces at the *beginning* of that 40th year, which is agreeable to the account in Tobit and yet consistent with all other events; and it would moreover hereby be exactly 40 years before the death of Esarchaddon, agreeably again to the 40 years of reign assigned to Artæus by Ctesias. Thus all authors, dates and events will agree harmoniously together; and here again we have another example of the benefit in point of harmony arising from my correction in placing the Jewish reigns right, that is, 3 years later than others, and thus consonantly with the respective Babylonian reigns in the æra of Nabonassar: for thus they leave the room, which is necessary for all the events connected with them, to follow one another in the order pointed out by some *historic* testimony or other, either that of Isaiah or some other. Prideaux however has delayed the evacuation of Syria by Senacherib until the 18th of Hezekiah, if not later, which is clearly 1 year too late, even although we should understand *third year* to mean exclusively of the 14th, this sense placing it no later than the 17th. But in regard to the death of that king he

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is still more erroneous, for he delays it until the 22d of Hezekiah, without giving any reason for thus delaying it 5 years later than others, who make his death to follow almost immediately after his return from Syria, i. e. on the 16th: his death however, by the Jewish reigns being fixed too soon in Prideaux, still falls only 2 years too late in the æra of Nabonassar, i. e. on 42d year instead of 40th; so that one advantage follows even from the above errors in that author, which is, that hereby the usurpation by Dejoces at least *precedes* the return and death of Senacherib, although indeed not exactly in the right years. Thus by his dates counteracting one another, notwithstanding that none of them are exactly right, yet it has happened fortunately in allusion to some lines of Pope, that

Dates balance dates, each error has its brother,  
And one mistake's corrected by some other.

For upon the whole by such counteraction the dates of the events in question are kept nearly in a state of equilibrium; but in most other cases any one error made by an author in one place produces many more and greater errors afterwards by augmenting one another, when they all tend the

same way. Yet even from such small errors of two or three years there is one ill consequence follows, for when sceptical readers find events placed by some writers a little *sooner* or else *later* than by others, they are too apt to conclude, that there is no good foundation for fixing them at one date rather than some other; and hence they join in with Bolingbroke and Collins in accusing all the chronology of these times of the utmost uncertainty. It is for this reason, that I endeavour to be exact to a single year, when the case will admit of it, and give my reasons, why it cannot be altered to either sooner or later; and although those cases may happen but seldom, yet if upon the whole the dates of events be but as certain as the events themselves, what more can rational men require?

It is for this reason then, it may be proper to add, that although most commentators fix the evacuation of Syria by Senacherib and his death also on the 16th of Hezekiah, not as in Prideaux one on the 18th and the other on the 22d, yet they seem to be a little *too soon* as well as Prideaux *too late*. For in the former case we must understand the 3d year of the prophecy to be reckoned *inclusively* of the 14th in which Senacherib entered Syria;

but

but for this sense I can find no good foundation, it seems to have arisen only from the following circumstance, that when the prophecy says "Ye shall eat *this* year such things as grow of themselves, &c." many persons refer the word *this* to the 14th year in which Senacherib invaded Judæa: but if it had been intended to have such a reference, it ought to have been inserted immediately after the relation of that act of invasion in the 14th, at *beginning* of 36th chapter, and not near the *end* of the following 37th, after Senacherib had returned out of Egypt, and only 6 verses before mention is made of his return to Assyria. Hence it follows, that *this year* does not refer to the *first* year of invasion, but only to the *first* of the three years connected with a Sabbatic year, as mentioned in the prophecy; but whether by that *first* of these three years was meant the 14th or 15th of Hezekiah, is left altogether in doubt; neither can it be determined by the words themselves of the prophecy, but from other circumstances only, such as these: if the invasion in the 14th began so early in spring as to be before they began to sow, then the 14th *might be* the first of the 3 years, and that in which they neither sowed nor reaped, as in a Sabbatic year;



but if the invasion was not until later in that 14th year, namely after May the harvest month there, then it *could not* be the *first* of the three years in question, and the 15th must be the first in which they could neither sow nor reap, the 16th would be the year in which they were to eat of what grew wild in the foregoing year, and the 17th or 3d year, the year of their freedom from Senacherib's army, and this early in the spring of that year the 17th before sowing time began. Under the veil then of the nature of a Sabbatic year and the two subsequent ones Isaiah describes as by a sign what the *duration* should be of the invasion, namely that it should last 2 full years, and no more; but still does not determine which year of Hezekiah's reign, whether the 14th or 15th, was considered by him as the *first* of the 3 years mentioned in the prophecy. Agreeably to the above sense of *this year* Tremellius, who generally adhered more closely than any translator to the meaning of the Hebrew renders it here by *anno uno* instead of *hoc anno*; the Hebrew often employing *unus* for *primus*: Leclerc also, although he translates by *hoc anno* yet explains them in his note to mean *primo anno trium annorum*, not the first of the invasion.

So

So also the Seder-olam expresses the prophecy, "Comedere vos anno uno sponte natum." C. 23. There is no sufficient ground then to suppose, that *this year* refers to the 14th rather than to the 15th year; and if the latter was the real fact, then the 16th could not be the 3d year, but the 17th, therefore the year of freedom from the Assyrians, in which they could first sow and reap: consequently the Assyrian army quitted Syria before the spring in 17th year, and this is the *latest* date of that event; but it is *possible* to have been 1 year *sooner*, though less probable, because it would place the return of Senacherib *earlier* than the revolt under Dejoces, which Tobit indicates as having *preceded* his return from Syria.\* This later date on the 17th

\* Most of the commentators moreover have erroneously pretended, as if this prophecy proved the 14th year to have been actually a Sabbatic year; but I can find no better foundation for this error than the former one; neither does it seem to be any way material whether it be true or not, for the prediction would contain the very same information of future events, either way: since in both cases the duration of the invasion would be limited by the 3 years in question, although Isaiah should have only described the time and circumstances of the invasion as being *similar* to what happens in Sabbatic years, whether the 14th or 15th were or were not

17th for the year of freedom leaves that whole year for Senacherib to *dwell* at Niniveh after his return, and even a large part possibly of the subsequent 18th year *before* his death; yet still that 18th year, the 41st of Nabon, might be reckoned as the 1st of his successor Esarchaddon, and thence 40 full years to his death. The 16th year therefore *might be* and probably was 1 year *too soon* for his death, but the 22d in Prideaux certainly 5 years *too late*. These different opinions it seemed expedient for me to consider, in order that the *accession* of Esarchaddon thus fixed by Scripture, might be as accurately determined as his *death* in the æra of Nabonassar. I have also adopted the *latest* date of the capture of Niniveh in the MSS of Eusebius, because it is confirmed by the positive testimony of Ctesias to be 62 years for the reigns of the last two kings, the æra of Nabonassar being neuter in this date; and also because this is the date in the MS of Pithæus, which is generally thought the most correct of any, as may be seen in a note to the Edition by Pontacus.

not Sabbatic years in reality. The Seder-olam did not understand either of them to be actually Sabbatic years.



# A N A P P E N D I X

## TO VOL. III.

OF

### Critical Observations on Books,

### ANTIENT AND MODERN;

#### CONTAINING

A Dissertation on antient Calendars, viz. the Macedonian, Athenian, Chaldean, Egyptian, and Roman, pointing out many capital Errors in the best modern Books on this Subject, and various Articles of new Evidence tending to the Improvement of it: together with accurate Tables of the Days of all visible new Moons during the first period of 76 Years, as instituted by Calippus; shewing also how the same Period, with the help of a few easy Corrections, may be applied as a popular Method to discover the Days of all visible new Moons in any other Age, past, present, or future, and thus serve for a perpetual Lunar Almanac;

#### TO WHICH IS ADDED.

A Discovery of the right Year, when that Eclipse happened, which is mentioned by Herodotus, as terminating the Lydian War; and a Calculation of the same by Mr. Hancock, in which M. Pingré's Errors are rectified in his Calculation of this Eclipse in the 42d Vol. of Hist. de l'Acad. Bell. Lettr. 1786.

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Non fumum ex fulgore, sed ex fumo dare lucem  
Cogitat, atque occulta adhuc mysteria pandit.

---

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of the life and character of  
George Washington  
and the other principal actors  
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By J. W. F. Johnson and  
J. W. F. Johnson

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J. W. F. Johnson

( L )

## AN APPENDIX

TO THE

### THIRD VOLUME.

---

*The use of the Calippic period proposed as a popular method to discover the days of the visible new Moons in all ages, past present and future.*

**A**S the chronologic subject allotted for this third volume is to make more accurate enquiries concerning the duration and principal dates of the *Fabulous* and also of the *probable* age in antient History, I may often in the investigation of this subject have occasion, through want of Historic evidence, to have recourse to the days when solar eclipses happened, and likewise to the days whereon the new moons fell, which commenced or terminated certain astronomic periods of time : but as the ascertaining such days is attended with intricate computations, when performed in a scientific way, and therefore for the accuracy of such computations Readers must in general depend altogether upon the accuracy

B

and



## 2      A P P E N D I X

and credit of the writer ; I therefore think it will be acceptable to them to be enabled to form some Judgement for themselves of the truth of such computations, by being presented with a more easy and popular method, whereby they may discover on what days of the Julian calendar any new moon and consequently any solar eclipse must necessarily have fallen in any past period throughout antient ages. Hereby a conviction of their own minds will better accompany the arguments of the writer. Now the method, which Calippus employed in the greek Calendars, in order to ascertain by a table the days on which all new moons must fall during every period of 76 years, and which was the last improvement made by the antients in this subject; the same will also serve for the purpose here required, if proper allowance be made for such inaccuracy as is well known to have subsisted in the method of Calippus : and thus it may be used as a *perpetual Almanac*, wherein any reader may in three minutes discover the day on which every visible new moon fell in any month and year whatever for 2 or 3000 years past or more ; and with sufficient accuracy for almost any question of history or chronology, although not of astronomy. The necessary information concerning the method employed by Calippus has been preserved in the tract of astronomy by Geminus ; but it has unfortunately happened, that the sense of the greek words there, and meaning of the writer, have been sometimes mistaken by the several authors and chronologers, who have attended

to this subject, Scaliger \*, Petavius †, Dodwell ‡ and Pontedera § ; and at other times they have supplied supposed deficiencies of information in Geminus with erroneous conjectures of their own, as well as supplied real deficiencies both in an inaccurate and inconsistent manner : So that although all those four authors pretend to appeal to the authority of Geminus for the rectitude of their own methods, yet they hereby necessarily have become inconsistent with each other, as well as each of them in many cases inconsistent with truth in regard to the meaning of Geminus, the real method of Calippus, and the agreement of their own methods with four dates by the period of Callippus preserved in Ptolemy's astronomy. Consequently although Scaliger, Petavius, Dodwell and Pontedera have each of them given us a table of the Calippic period, yet not any one of those tables can be depended on, or applied to any use whatever in history or chronology. But it is not my intention in this *first Dissertation on antient Calendars*, to point out the many mistakes into which those several authors have fallen, this would be too large an undertaking at present ; I only mean to give a *general sketch* of the method, in which I myself have proceeded in the construction of the subjoined more accurate Tables of the Calippic period for the

\* De Emendat. Tempor. lib. 5.

† Doctrin. Tempor. lib. 2. c. 19.

‡ De veter. Cyclis Græcorum Dissert 9.

§ Jul. Pontederæ Antiquitat. Græc. Patav. 1740, pag. 242.

## 4      A P P E N D I X

first 76 years after its introduction: this will in course display at the same time *some* of the chief errors of those former writers, while it also explains the *general nature* of the subject and prepares Readers to comprehend more easily what I chiefly have in view in this Essay; which is, to supply them with *an obvious method*, whereby they may apply these Tables to discover the day of any new-moon in any year required, past, present or future. I have moreover thrown this tract into the Form of an *appendix to the third volume*, in order that it may be the more ready at hand for readers to have recourse to there in order to verify for themselves the day of any new moon or solar eclipse, which may be made use of by myself in this volume concerning any chronologic subject.

*Explanation of some of the most necessary articles relative to the mode of constructing rightly Tables of the calippic period, and agreeably to which the subjoined ones are executed; whereby they will become capable of being applied to use, which the calippic Tables of former chronologers are not.*

THE *Computation* of new moons by these Tables *begins* in the year 330 before the vulgar æra of Christ, as is noticed in the second perpendicular column of the 1st Table; for that was the *year*, as all writers agree, on which Calippus placed the commencement of  
his



his cycle or period of 76 years : in the same horizontal columns with the years before Christ are marked the cotemporary dates by the Olympiads and the æra of Nabonassar. But in regard to the *season* of that year 330 or the *month* in it, at which Calippus made his period to commence, no sufficient information concerning this article has been transmitted to us by antient writers : Petavius and others have indeed confidently asserted, that it began, like the period of Meto, with the new moon nearest to the summer solstice, either before or after it. But Scaliger and Dodwell assent to this only so far as respects the popular use of the calippic period in Europe by the Athenians and Macedonians ; for with regard to Asia they maintain, that it commenced there at the autumnal Equinox : but that the calippic period thus differed in regard to the *season* of its commencement in different nations there is not the least evidence to prove, as Petavius has properly observed ; and Geminus certainly gives not the least hint of any such variety in it ; therefore Petavius has totally rejected the idea of any such fact, and apparently also with good reason. Thus much however may be demonstrably proved from some dates by the calippic period preserved in Ptolomey, that it must have had its commencement *later* in the year than the summer solstice ; although indeed not any one of those writers have adverted to this, but on the contrary they have all contended or acquiesced in an opinion, that some passages in Ptolomey prove it to have commenced at the summer solstice, which in fact

## 6      A P P E N D I X

none of them do : and the real truth is, that none of those dates or passages in Ptolomey go so far as to prove at what precise month or season the calippic period commenced, but only that it must have commenced *later* than the summer solstice, yet not *so late* as the winter solstice ; hence it very probably follows, that it commenced at the Autumnal Equinox. An examination of these proofs would lead us at present too far away from our proposed object ; it is sufficient, that I have pointed out my chief reason for making the calippic period to commence with the Athenian and Macedonian months *next after* that equinox, and this at Athens as well as in Asia (in case it was ever in use in either) although indeed in contradiction to the determinations of Scaliger, Petavius and Dodwell. Hereby the first Macedonian month *Dius* would commence in the first calippic year (the year 330 before Christ) at sun-set on the 24th of October, which would be at the appearance of the visible new moon ; for the true new moon fell about midnight between the 23d and 24th ; thus the month *Dius* could never go back farther (as may be seen in my Tables) than to the 26th of September, therefore would never anticipate the equinox : this was a considerable improvement upon the metonic period, in which the months that *generally* fell next after the equinoxes and solstices did *sometimes* anticipate them not less than 10 or 12 days. So that this was also a *proper* moon for the new calippic period to commence at, as well as probably the *real* one. Moreover, hereby the first Macedonian month

*Dius*

*Dius* would then coincide with the first half of the first Egyptian or rather Chaldæan and Persian month, which began in that year 330 for the last time on Nov. 14 and afterwards would gradually commence sooner and sooner; and thus would gradually coincide more and more with the first Macedonian month *Dius*: this was probably one object in the view of Calippus, Asia and Persia being in the year 330 first made subject to the Macedonians by Alexander; to whom the introduction of this new calendar (which would thus harmonize the before discordant Athenian, Macedonian and Persian calendars) was very probably intended as a compliment by Calippus, and as expressive of Alexander's being now Lord and master of all those three great nations; an introduction of a foreign calendar being in those ages a general consequence of conquest, and as it were a badge of subjection.

It must be remembered however, that I do not pretend to have fixed the first month of the calippic period at the autumnal equinox as a matter of absolute certainty, but only as having more and better evidence in its favor, than the foregoing authors have been able to produce in favor of the summer solstice; of which the following fact may be considered as some proof: for not any one author, who has hitherto writ upon this subject, and has placed the commencement of the calippic period at the summer solstice, has been able to make the dates of such projected Tables coincide with *all* the 4 Athenian dates by the calippic period found in Ptolomey; a certain



## 8 A P P E N D I X

evidence, that there must be errors in their proposed plans somewhere, (in case the text of Ptolomey be not corrupted) but whence those errors arise is matter of difficulty to be ascertained ; and unless it can be better ascertained, it may *possibly* have arisen from that circumstance of their having placed the commencement of the calippic period at the summer solstice instead of the autumnal equinox.

Another doubt is, whether the calippic period was ever actually in current and *popular use* either at Athens or in Macedonia or Asia ; in regard to this the opinions of the learned have differed ; with respect to myself I can truly say, that I have never met with the least proof of its having been adopted as a popular calendar *any where*, but rather with evidence to the contrary : and it is no way improbable but that Calippus might have been disappointed of his proposed purpose, which possibly was not merely to rectify the metonic period at Athens ; but that he had a larger view, namely to harmonize the discordant calendars of the Athenians and Macedonians, so as that the same months of each might afterwards always correspond with each other month for month, and day for day. In this project however he probably failed (as has been the case with many excellent proposed reforms in all nations) so that his calendar was never apparently adopted except by a few astronomers and scientific men ; to whom it might possibly serve the same purpose in regard to dates as an universal Language would do in correspondence : For both the Athenians and  
Mace-

Macedonians seem to have still continued to the very last their former national attachments to their own more antient and more erroneous methods; which produced so great a discordancy between the respective months of each, as that the same Athenian months are found in History to be mentioned as coincident with three or four different Macedonian months in different ages. Now from what cause these discordant facts could arise has been the subject of much enquiry among the learned; the only cause to which all authors have hitherto had recourse, has been a mere supposition of their own, that in different ages the Macedonians arbitrarily altered the places in the seasons, which their months had before occupied; so that *Dius* for instance was made by them coincident with October in one age, and in another was arbitrarily made to be coincident with November and so on: Dodwell after others supposes no less than three such arbitrary changes made by the Macedonians in the places of their months; as may be seen at p. 394, *de veter. cyclis*. But for such a supposition there is not the least evidence in History; neither is there the least necessity for having recourse to such arbitrary imaginations, which with the aid of others of a similar kind give one too much reason to consider their writings on such subjects in no better light than that of learned romances: And the discordancies between the Athenian and Macedonian months in different ages may be accounted for by a more natural and probable cause; for they had apparently no other origin than

than this simple fact, that the Macedonians continued untill the very last to employ the old octennial period, which had been almost universally in use throughout Greece, before Meto introduced his period of nineteen years at Athens in the 87th Olympiad. It is sufficiently intimated by Cicero, that this period was in use in some cities of Sicily or elsewhere during his own time; for he says, that the Greeks sometimes *added* to their months a day or two by intercalation to make them correspond with the new moons \*; now there could be no occasion to *add* a day or two except in the octennial period, because it was too *short*; whereas the metonic was too *long*, therefore in this they always *took away* or cut off *one day* when wanted: Cicero therefore could be speaking only of the octennial period, when he speaks of *adding* a day or two; and if this was subsisting in Sicily down to even Roman times, why not among the Macedonians also both in Europe and Asia? Diodorus indeed says, that *many* of the Greek states had adopted the metonic period, but he does not say *all* †; and

\* “Est consuetudo Siculorum cæterorumque græcorum, quod suos dies mensesque congruere volunt cum solis lunæque ratione; ut nonnunquam, si quid discrepet, eximant unum aliquem diem, aut ad summum, biduum ex mense (quos illi *exemptiles* dies nominant) *item nonnunquam uno die longiorem mensem faciunt aut biduo.*” Orat. 7. in Verr.

† “Græcorum plerique Metonis rationem amplexi sunt.” *Lib. 12. Olymp. 87. 1.*—“Octaeterida verum annum magnum esse pleraque græcia existimavit—multæ in græcia religiones hoc intervallo temporis *coluntur.*” Censor. c. 18.

Cen-



Censorinus informs us of the great prevalence of the octennial period in Greece, with an intimation that it was even then still subsisting there, at least in sacred rotations of time. Why then should we without any evidence suppose with Chronologers that the Macedonians had adopted the Metonic or Calippic period, when we have no historic nor either internal or external proof of it; but on the contrary have one strong internal proof in favour of the octennial period, besides various other proofs? For it is the *necessary* consequence of the use of the octennial period, that every Macedonian month must be successively coincident with three different fixed Julian months, and *possibly* with no less than 4 lunar and moveable Athenian months in the course of every period of 152 years, i. e. of 19 octennial periods. It is truly wonderfull therefore that although all our chronologers knew this consequence, yet that not any one of them should have adverted to such an obvious and natural solution of the difficulty, which had perplexed them; but should have rather run wild with the roving of mere conjecture. There are also several other circumstances preserved by history, which tend to confirm the use of the octennial period by the Macedonians, of which I shall at present mention one, in order that I may not advance any thing new, and contrary to the opinions of others, without suggesting like wise weighty evidence in it's support, of an historic and external as well as internal nature. Now it is well known, that the octennial period was the first, which was employed by  
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the christians in order to fix the time of observing Easter; this was very natural, in case the octennial period had been in vulgar use among the Macedonians in Palestine, where Christianity arose, or even among the Macedonians at Alexandria in Egypt, where the Christians of the greatest erudition resided: but it is altogether unaccountable, in case the metonic or calippic period had been in popular use at either of those places; for as the first Christians had but little astronomic or other erudition, how strange is it, that, instead of falling into a similar use of the metonic period along with the Macedonians (among whom they lived, and which they daily saw employed around them) they should on the contrary send to some remote and obscure cities of Greece for an antient period exploded almost every where, as chronologers would persuade us! It was not untill about the year 300, that Anatolius first introduced among the Christians at Alexandria the use of the metonic period of 19 to find the time of Easter: and thus by a strange kind of cross-purposes the unlettered Christians of Palestine sent us far as to Greece for a foreign period ever before unknown to them; while on the contrary the more learned Christians of Alexandria first employed the vulgar metonic period though in common use both in Palestine and Egypt, as our chronologers pretend. But in case the octennial period was in vulgar use every where among the Macedonians, then these events took place in a natural and probable manner: for then the fact would be, that the more literate Christians at Alexandria, where the academy  
of

of astronomy was renowned, at length obtained knowledge enough of that science to discover, that the metonic period in foreign use at Athens would be a more accurate method for their purpose of fixing Easter, than their old popular octennial period established among themselves and the Macedonians in Palestine; from whom the first unlettered Christians had borrowed it, and carried it to the Christians at Alexandria. Still farther, the very first improvement of the octennial period attempted by the Christians, was that by Hippolitus, who in his Paschal cycle, which begins in 222, only doubled the octennial period, and formed one of 16 years\*; of which no person hitherto has been able, notwithstanding the pretences of *Blanchini*, † to discover any real utility; for all its former imperfections would still remain when thus doubled: it seems indeed to have been rather intended for historic purposes relative to past ages and in no very accurate manner, than to be of any real or practical use in future ones; accordingly we have no knowledge, that it was ever received into use by the Christians, but at least it was superseded soon after by Anatolius's introduction of the metonic period of 19 years. Lastly, there was another period of 84 years in use among the antient Christians, and if Epiphanius is to be credited, among the Jews also; yet more probably however among the dispersed Jews of the 3d and

\* See Crit. Observat. vol. I. p. 279.

† De Calendario Cæsaris et Paschali Canone S. Hippolyti a Fr. Blanchino Romæ 1703. p. 93.  
4th



4th century than as Epiphanius erroneously supposes during the time of Christ (for they then doubtless ascertained their lunar months by visible observations of the new moons; and it was not untill after their dispersion by Titus and Adrian, that they began to use scientific cycles or periods) so that if this period of 84 was ever in use among the Jews, this circumstance may be considered as a proof of its late origin (*Epiph. contr. Alog. c. 26*). Now this is the only period, which indicates the least knowledge among the Christians of the calippic period; for this period consists of 84 years, which is a combination of the 76 years of Calippus with the 8 years of the old octennial period; so much were the Asiatics, whether Jews or Christians, attached to the octennial period! an attachment truly wonderfull, if it had never been in vulgar use among the Macedonians in Palestine, and had also been almost exploded even in Greece itself! This period is however liable to the same error as the octennial one, by being too short, so that it contained no improvement upon the calippic period; and that it was in early use among the Christians there is no testimony to prove: it might possibly have arisen still later than even the year 300 and the introduction of the metonic period, as its more complex nature does indeed render probable. I have entered thus far into this subject, in order to give readers some idea upon what foundation the following *conclusion* and information is built, namely, that they must not suffer themselves to be led into an erroneous supposition, that the Macedo-  
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nian months, which in my Tables I have made coincident with Athenian ones, were in fact *actually so coincident* in times *before* the commencement of the calippic period in the 330th year before Christ, nor yet that they were actually so coincident *afterwards* (unless the calippic period had been adopted by them, which as I conceive it never was); but they are only *to conclude*, that the Macedonian and Athenian months in my Tables were thus *actually* coincident in those 3 years, in which the 3 dates by Macedonian months occur in Ptolomey. Now this fact we learn from the dates of those celestial observations by Macedonian months in Ptolomy about 80 or more years later than that year 330; and these enable us to trace back the position of Macedonian months at the commencement of the calippic period; for which purpose these calippic tables are of excellent use, even although the Macedonians really employed the octennial period: for by reckoning either forward or backward in the tables from eighth year to eighth year, we can discover how every Macedonian month must have fallen in their octennial period either *before* or *after* those dates by them, which are so well ascertained in Ptolomy. If we reckon backward, then every Macedonian month will be found in every eighth year to fall 1 or 2 days *sooner* in the Julian months, and on reckoning forward they will fall 1 or 2 days *later* in every eighth year; the month *Dius* thus proceeding, for instance, from October 24 to Oct. 26 in the subsequent 8th year, then to Oct. 28, then to Oct. 29, and so downward: but contrary-wise

wise when we reckon backward; because the octennial period is too short by a day and half or more in every 8 years \*. When by thus falling later and later the months had varied from their original places in the Julian months to the amount of 30 days, (which would be at the end of 152 years) the Macedonian months were then in the octennial period, by the omission of an intercalary month of 30 days, brought back once more to their original places in the fixed Julian months, and then *Dius* would commence again on October 24. We may thus by means of those 3 dates in Ptolomey be able to discover on what Julian days any Macedonian month commenced in any age, within a day or two, in case they really used the octennial period: so that these tables are just as usefull to discover Macedonian dates under the octennial period as under the calippic; a discovery of lost intelligence no other way ever possible to be recovered! I said within a day or two, because as in the octennial period there were always 1 or 2 days intercalated at the end of every 8 years, we are not able to discover which of these two units were *actually* intercalated at any given time, whether only 1 day or 2; therefore we may err to the amount of 1 day, but seldom more, for the octennial period could never vary from the times of the visible new moons more than 1 day or 2 at most; it being at the end of every 8 years corrected by visible observation of the new moon; and 1 or 2 days intercalated in case the new moon did not appear.

\* See Tabl. I. at the calippic years, 1. 9. 17. 25.  
Hence



Hence therefore it follows, that wherever the octennial period was in vulgar use, there was a *necessary progression* forward to the amount of 30 days in all the lunar months, and thus every Macedonian month must have necessarily during the course of every period of 152 years have become coincident with 3 different Julian months: for all Lunar months must always in every case and under every period, whether the octennial, metonic or calippic, have been successively coincident with at least 2 different Julian months and partly with 3 months in every 3 years; but under the octennial period they would be coincident with 3 Julian months in different ages of that period and partly with 4. For as the Macedonian month *Lous*, for instance, coincided with *July* and *August* at the beginning of a period of 152 years, it must on account of the abovementioned necessary progression forward coincide at the end of that period with *August* and *September*, i. e. it must advance forward a whole month. Altho then there should be no sufficient historic information transmitted down to us to remove the *doubt* abovementioned, whether the calippic period was ever received into vulgar use among the Macedonians in Asia; yet hereby we may be at least able to solve that much debated question concerning a passage in an epistle of King Philip quoted by Demosthenes in his oration *de corona*, which has been considered as the *re plus ultra* of all critics and chronologers: but which by means of these calippic Tables will admit of an easy solution. For in that Epistle the Macedonian month

C

*Lous*

*Lous* is mentioned as being coincident with so late an Athenian month as *Boedromion*: Now this fact all critics and chronologers have considered as almost impossible, or at least by them inexplicable; Corsini thought himself so assured of its impossibility, that he pretends to have demonstrated, that *Boedromion* must be an error in the text for *Hecatombaion* \*. Thus the text of ancient authors is corrupted through the rash conjectures of learned Romancers †: for if the octennial period prevailed in Macedonia (and the fact now in question was before the institution of the calippic period) I have shewn, that *Lous* must necessarily, near the end of every period of 152 years, coincide with *September*; with which Julian month the Athenian *Boedromion* was generally coincident. Now that the date of that Letter of Philip was near the end of a period of 152 may be proved by the 3d Macedonian date in Ptolemy; where it appears, that *Lous* began as early in that year as July 3: and on tracing that moon backward in my tables from 8th year to 8th year, as described above, it will appear, that

\* “*Operæ pretium me facturum putavi, si hæc omnia subtilius inquirens Demosthenis locum certissimè vitiatum esse demonstrarem, et non Loun Boedromioni, sed Hecatombæoni conjungi potuisse ostenderem.*” *Fest. Attic. tom. I. 140.*

† A similar corruption has actually been admitted into all Editions of Livy, where he describes Numa's construction of the Roman year: *all* the MSS. read 20 years, in which the Roman period was completed; nevertheless *all* the editions have admitted 24 years on the credit of the unintelligible account of it by Macrobius.

*Lous*

Lous must have commenced in an octennial period on June 17 about 30 years *after* the date of Philip's letter; which was as early as Lous ever could probably anticipate the solstice\*. Consequently a *new* period of 152 must have *begun* in the interval between the date of Philip's letter in the 339th or 340th year before Christ and the 3d Macedonian date in Ptolomy, about 100 years afterwards; neither could this *new* period have begun above 30 or 40 years later than Philip's letter: so that when it was writ, which, we see, was near the *end* of a period of 152, the Macedonian months must have then commenced *late*; and if the Athenian months in the *same year* commenced *early*, as they very easily might have done, nay *must* in case the Macedonian intercalary month was inserted at the Autumnal equinox, and the Athenian not until the winter solstice in the *same year*, i. e. if the Macedonian intercalation *preceded* the Athenian; it hence follows, that Lous and Boedromion might have *then* both of them coincided with the *greater part* of September in several years. Nevertheless, altho such a coincidence must be often the *necessary* consequence of the retention of the old octennial period by the Macedonians; yet the very learned Dr. Taylor in his notes on Demosthenes has implicitly followed the arbitrary suppositions of Scaliger, Petavius and Dodwell, that the Macedonians made two or three *voluntary* changes in their calendar in regard to the places occupied by their months; just

\* See the 21st calippic year, which was the 30th or 31st year after the date of Philip's letter.



as if romance was contagious in Literature: but of such pretended changes there is not the least evidence in History; nor yet that the Macedonians even ever exchanged the old octennial period for the metonic or calippic either in Europe or Asia, but many proofs to the contrary. Such proofs to the contrary however, since no *express words* concerning this subject are to be found in History, must be drawn from *indirect notices and indications* accidentally afforded us by various writers, while treating on other subjects; just as a Physician investigates the unseen disease within by the outward *indications* and visible *symptoms* of it. I have already pointed out several such *indications*, and will here subjoin another to the same purport. Both Eusebius (*Dem. Evang. lib. 8.*) and Jerom (*Comm. Dan. c. 9.*) have preserved a passage of Africanus, who lived about the year 222, and who was employed as Ambassador to Heliogabalus in favour of rebuilding Nicopolis in Syria; a proof, that he was no stranger to the concerns of Asia. Now in this passage Africanus computes the 490 years in Daniel by the number of months and days contained in the octennial period; and expressly informs us at the same time, that this period was employed by both *Greeks and Jews*, who (he adds) *made intercalations of 3 months in every 8 years, which amounted to 90 days in all, i. e. to 30 days in each month* \*. But it was only in the octennial period, that 3 months were intercalated in 8 years; for the metonic had 7 months

\* "Græci et Judæi per octo annos trium mensium intercalationes faciunt, i. e. 90 dierum."  
intercalated

intercalated in 19 years, and these were indifferently either of 29 or 30 days: whereas the 3 months, of which Africanus speaks, made 90 days, therefore must have each consisted of 30 days; a manifest proof, that he was speaking *only* of that octennial period, by which he computed the 490 years of Daniel. Who then could *the Greeks* be here referred to by Africanus? He has also joined *the Jews* to them; he must probably have meant *those Greeks*, who were most connected with the Jews, and himself, i.e. *the Greeks of Asia*: from these the Jews apparently must have borrowed the use of the octennial period after their dispersion by Adrian about the year 135; that dispersion having rendered the use of a scientific cycle necessary to ascertain the day of their passover in the different nations to which they wandered, instead of their former practice of visible observations of the new moons in Palestine. The first Christians, as we have seen, had borrowed the use of the same octennial period for the same purpose; until Anatolius had learned and introduced the better method of the metonic cycle in use at Athens: and this common adoption of the octennial period, both by the Christians and Jews of Palestine in their infant and dispersed states, contains a strong *indication*, that it had some common and obvious origin; such as the *general* use of that period by *the Greeks in Asia*; among whom these illiterate, fugitive and persecuted bodies of men resided. This use of the octennial period by the Jews after their first dispersion (and until they also about the year 300 discovered and introduced that

metoric period among themselves, which they continue to employ unto this day) is confirmed by Epiphanius, who says, "Tota illa periodus octaeterica dies sibi vendicat 90, qui menses solidos efficiunt tres—in his tribus intercalari-bus mensibus Judæorum Pascha a Christiano-rum Paschate diversum est;" *Contr. Audian. Har.* 70. *sect.* 13. By the Christian mode of ascer-taining the Passover he means that metonic one in use in his own time after the year 300; there are also sufficient proofs in the same passage that before 300 the Christians had employed the octennial period like the Jews\*. Thus I have given a *specimen* how these calippic Tables may be made to contribute to purposes of criticism as well as chronology, and this as well in regard to the octennial period as the calippic itself. A variety of passages in Hero-dotus, Dionysius, Diodorus, and other antients may by the same means be illustrated, which are at present involved in darkness; and hereby they may be defended against the desperate hands of editors and rash pens of commenta-tors: at some future opportunities I may enter farther into this subject of applying these tables to explain the meaning of antient authors; but altho thought is quick, yet time is more hasty, writing tedious, and printing expensive.

Upon the whole then, with respect to these subjects of the *coincidence* of Macedonian months, as mentioned in history, with so many different Athenian months, and the *necessary progression*

\* See also Euseb. Hist. Ecclesiast. lib. 7. c. 15. and Sozomen. Histor. lib. 7. c. 18. and Beda de Ration. temp. c. 44.



forward of the octennial period to the amount of a whole month, one should have thought, that the very first and most obvious idea, which would have occurred to chronologers, was, that the above facts were *cause* and *effect* to each other; since it was expressly in order to remove such an ill consequence in the Athenian calendar caused by the octennial period, that the metonic period was introduced at Athens: for in regard to keeping the commencement of lunar months closely adjusted to the visible new moons, the octennial period accomplished this even more accurately than the metonic, at least at the beginning of every 8 years. If then the *ill effect* of the octennial period be found in history to have still subsisted among the Macedonians, after the Athenians had removed it from their own calendar; what can be a more reasonable, natural, and probable conclusion, than that the same *cause* still existed among the Macedonians, which had before existed among the Athenians; and to remedy which they purposely introduced the metonic period? more especially since we have also found this *cause* to be an adequate one, by its being able to produce and account for the very worst *ill effect* and the greatest discordancy to be any where found in History. As there is no express historic and external evidence extant on either side of the question, the above circumstances must be reasonably thought to contain strong internal evidence in favour of the octennial period; especially when they are assisted by the historic *indications* and indirect *notices* to the same purport, which I have pointed out

C 4

already,

already, and to which many others may be added. But it often happens, that the most easy method of removing obstructions and difficulties is the very last, which is attended to; because when learned authority has once introduced its own chimæras, it draws after it all other readers and writers into its own vortex: hence literary men are often found as fond of wonderful revolutions in prophane antiquity, as Dr. Priestley is of extraordinary deviations from original Christian opinions; and scarcely also in other subjects does any man raise his head above the level of the multitude, but some how or other he seems impatient to thrust it even among the clouds, where confusion and darkness surround it. Such was the case with the immortal Plato, and such is the case not only with many metaphysicians, but with the learned in general in many other sciences. It is now high time, however, for all denominations of men to become more willing to lay aside both antient established prejudices and modern imaginations of mere ingenious novelty, that they may the better employ their reason to bring real truths to light, if possible; but at least not to obscure them still more, and thus to darken knowledge with many words. Certain I am, that no error of my own, which comes under my cognizance, shall stand a moment uncorrected to mislead others: but Dr. P. in his *defence* for 1787 has not been able to prove a single one. There is in fact too much reason to lament, that in all ages there have been no greater romancers in the world than men of learning; nor any who could tell a strange tale of a voyage

age to the Moon or to the Island of Laputa with a more steady countenance of self-confidence, and without *blushing on reflection* afterwards at the wildness of their own fancies, or at the errors into which others may thence be led, by taking up with learned visions instead of real truths. It must nevertheless at the same time be confessed, that there is something very seducing in the force of imagination; and whatever object is pursued with ardour, is generally pursued also with a certain degree of *Extravaganza*: learned romances therefore may be in some measure excusable by their affording amusement to the reader as well as writer, *Levis hæc insania quantas virtutes habet!* But will the same excuse hold in favor of Unitarian Romances, when built on no better foundation than such errors in facts, errors in quotation, errors in syntax, and errors in reasoning, as ought not to mislead an intelligent man a single moment?

The next doubt in regard to the calippic period respects the *order*, in which the intercalary years followed each other: concerning this no evidence has been preserved; it has been presumed indeed, tho without sufficient reason, from the words of Geminus, that Calippus made no alteration in this respect from Meto's order of intercalation; but this is disproved by some of the calippic dates in Ptolemy, as Scaliger also has observed. I have therefore in this article adopted the order of intercalation proposed by Scaliger, except in one not very material instance; and for this reason, because it is in fact the only *possible* method,



method, which will make the 4 Athenian dates in Ptolomy by the calippic period fall on their right Julian months, and yet at *the same time* make the 3 Macedonian dates there fall on their right Julian months likewise. So that *in case* those 3 Macedonian dates were really reckoned by the calippic period, it would hence follow *with certainty*, that the order of intercalation, which I have adopted, must indisputably be the right one; but as I have given reasons to suspect, that those Macedonian dates were reckoned by the octennial period, some different method might in consequence be proposed, which would agree as well with the 4 Athenian dates as that in my tables: yet none however so good as this of Scaliger in one point, that is, of preventing the commencement of the Athenian months from anticipating the solstices and equinoxes; but then we are not certain, that Calippus paid any regard to this object, any more than Meto or the octennial period did before him.

Another doubt respects *the particular Macedonian month*, which was by intercalation made to become double; of this point also no information has been preserved by History. I have therefore made it to be the last month in the Macedonian year, namely, *Hyperberetaus*; yet not from any confidence of its being really so, but only because it was necessary to fix on some one month or other: The Athenian year however was not intercalated at the end of it, but in the middle; and so might that of the Macedonians. In fact there is also a slight *indication* in Plutarch, that *Artemisus* was the intercalated month;

month; for at that month Alexander on his passage of the Granicus inserted an extraordinary intercalation, in order to make the month Dæsius fall *later*, and thus to delay it until he had passed the River: but Alexander was such an extraordinary man, that one can form no judgement of ordinary things from his conduct.

It only remains then to explain the nature of the *exemptile* days, as they are called (see note p. 10.) The Macedonian and Athenian Calendars contained 12 months of 30 days each, which made 360; but these being too many for the 354 contained in 12 Lunar months of  $29\frac{1}{2}$  days each, the Greeks at first made 6 to be of 29 days only and 6 of 30 alternately during the octennial period; that is, they cut off or *took away* (as *exemptilis* expresses) 1 day out of every other month and this always *the last day*. But Meto perceived, that hereby they took away *too much*; and made the octennial period *too short*, because the moon is something more than  $29\frac{1}{2}$  days in its mean period: instead then of the former method, which took away, or omitted 1 day in every 60, he computed that only one day ought to be omitted in every 63 days; and this wherever it happened to fall, whether at the beginning, middle or end of a month. Now the figures marked in my tables thus (25) point out with what Julian day these *exemptile* days were coincident, which were thus omitted to be reckoned; so that (25) means, that it was the Athenian or Macedonian day coincident with the 25th of December, for instance, or with any other Julian month there mentioned: and this day

day whenever it shortens the *subsequent* Greek month is placed *under* the name of the respective Julian month; but *above* it, in case its effect is to shorten the *preceding* Greek month. These exemptile days ought to be placed invariably throughout the 76 years on every 63d day, reckoning from Oct. 24 *exclusively* in the 1st year; and if they are not so placed, it is an error, which wants correction: but I presume, that they are placed right, because I perceive, that they fall on the right Julian days in the last month of each of the 4 quarters. As Posidon then commenced in the first year at sunset on Dec. 23, the 25th of December will be the 2d Athenian day; now this was *omitted* or cut off by calling it the 3d. It must be observed however, that there is an error of one day made in placing these *exemptile* days, which runs thro all the 4 tables until June 5 in the 70th year; where it is first corrected with a pen along with another error, and the corrections continued unto the end of the tables. For the case was, that the 24th of October in the 1st year was at first *included* in *Dius* which made Dec. 25 to be the 63d day; but afterwards it was found, that this would place the commencement of the Athenian months 1 day too soon for the 4 Athenian dates in Ptolemy: the months are therefore to be reckoned only from *sunset* on the 24th of October, and that day must be thus *excluded*; which will make the 26th of December to be the 63d day and the *right exemptile*, not the 25th. The same error consequently runs throughout the 4 tables, and each *exemptile* ought to contain




rain *an unit* more than it does; but I have examined and also corrected with a pen every place, where this can cause an error in *the date* of the respective Julian month, at which any new moon *begins*: readers however ought still to guard against the effect of this error at the 7 Greek dates from Ptolomy, marked in the tables 1 *Athen.* 2 *Athen.* &c. 1 *Macedon,* &c. It must be noticed moreover, that those *Macedonian dates* are placed in the printed tables a whole quarter *too late*, owing to an experiment made concerning them, which was afterwards forgot to be rectified: so that *the right Macedonian dates* are those, which are writ with a pen. If any other errors should be discovered, I should be obliged to those, who will inform me of them by a letter left with the Publisher.

The 5 months at the beginning of the 1st Table marked in Italics *June 29, July 29, &c.* express how the Athenian months commenced in the metonic period, immediately *before* the calippic period began: the error which had been then generated in the metonic period made the new moons fall 1 day *too late*; which error Calippus corrected and raised them up from Oct. 25 to Oct. 24.—*Bis.* marks the Julian year, which would be a *bissectile*, in case the Julian years were continued backward from the time of their institution by Cæsar—13 *Interc.* means, that the 13th year of the metonic period is proved *with certainty* in Ptolomy to have been an intercalary one; the other intercalary years are doubtfull even in the metonic period

period as well as calippic. I think, that I have now explained every thing which requires explanation in order to comprehend the *method* of these tables, tho much is requisite to be added in order to explain and prove the *principles* on which they are computed.

Having *cautioned* Readers against concluding, that the Macedonian months occupied *precisely* the same places as are assigned to them in my tables, at any other times than *during* those identical *three years*, in which the three Macedonian dates from Ptolomy are marked: and also *cautioned* them not to conclude, that the Macedonians really employed the calippic period, because Scaliger has been able to reconcile those *three* Macedonian dates to the same order of intercalation as the *four* Athenian dates, which were certainly reckoned by the calippic period (this having been effected only thro that *particular order* of intercalary years adopted by me from Scaliger, which we have no sufficient authority to pronounce the real method employed by Calippus) for those *three* Macedonian dates by means of a different mode of intercalation may be equally made reconcileable to an octennial period; it may still be not improper to add the following farther *caution*, lest it should be supposed that I have begun at the wrong end. I formerly made a kind of promise to give some less perplexed account of the Macedonian calendar, than had been done by former writers, such as Scaliger, Petavius, Usher, and Dodwell; and to clear it from the difficulties and improbabilities,



probabilities, in which it was at present involved \*: accordingly this is my *first dissertation* on this subject, and a necessary preparation to farther progress in it by myself or others. But some incautious readers may be apt to suppose, that by thus beginning with the *latest state* of the Athenian and Macedonian calendars, I begin at the wrong end: to such I have to observe, that I adopt this method for the same reason, that I formerly began an investigation of the principles of chronology with *so late* a date as the invasion of Greece by Xerxes. It has indeed been the too general practice of writers to begin their accounts of antient calendars almost with the beginning of the world, but I must confess myself not to be so perfectly initiated into the mysteries of antiquity; and the Public has been already favored with a sufficiency of Romances: I begin therefore where that *first information* begins, which we can depend upon with any degree of *certainty*; and this is with the 7 dates by the Athenian and Macedonian Calendars in Ptolomy; these we can with certainty fix to their right correspondent dates in the Julian Calendar by means of the cotemporary dates in the æra of Nabonassar, which Ptolomy has given us likewise. Here therefore it is that *investigation* must begin, and afterwards go back in a retrograde course to more antient ages, as far as facts and reasoning can lead us: for whatever is most remote, is generally the least known, and the most difficult to be disco-

\* See Critic. Observ. vol. I. p. 338.

vered



vered with accuracy. Accordingly this was the method, which Dodwell himself had proposed for his own enquiries \*, and if he had strictly adhered to his own proposal (which he did not, for he has treated of the metonic before the calippic period) he would not perhaps have fallen into so many errors of his own imagination: and the real truth is, that not any one date during the metonic period has been preserved, which we can fix to its correspondent Julian day, not yet any even by the calippic period more antient than those in Ptolomy. The ill consequences of the method of beginning investigation in more antient times will be sufficiently conspicuous from the following single example; for Scaliger, Petavius, Dodwell and Pontedera have differed no less than five days in fixing the commencement of Meto's period: Pontedera has made it to have begun at sunset on the 12th of July and thus to be chiefly coincident with the 13th agreeably to the *supposed* but not real meaning of Diodorus †; Scaliger has made it to be chiefly coincident with the 15th; Petavius with the 16th; and Dodwell supposes it to have begun at sun-set on the 16th and thus to be chiefly coincident with the 17th. Disagreements to such an amount as these would destroy all possible accuracy in the tables of the calippic period; and it has accordingly followed hence, that those compiled by the above authors could never be applied

\* “Plerique a temporibus orsi antiquissimis gradum inde fecerunt ad tempora recentiora—sed non quo antiquiora sunt, eo sunt etiam exploratiora, sed quo *experimenta plura* exhibent, quæ possint cum Formâ Julianâ componi.” *Vet. cycl. p. 3.*

† Apud Olymp. p. 87. 1.

to any use in ascertaining the new moons. But by investigating this matter in a retrograde course from the 4 Athenian dates in Ptolomey I find; that Scaliger was right; and that the others, while they pretended to correct him, did in reality corrupt his method: yet Scaliger has however made other mistakes, which equally set aside all use to be derived from his own method. All this has been the consequence of depending too much upon their own fallacious conjectures and reasonings; instead of searching out certainties and facts by investigation backward from the 4 Athenian dates in Ptolomy, and agreeably to one invariable mode of computation throughout, such as is pointed out by the words of Geminus, when rightly understood. So that not any one of those four authors has found *all* the four Athenian dates to agree with their own pretended tables of the Calippic period; neither in fact do those authors always agree with themselves, any more than with each other or with truth.

*Method of finding by these Calippic Tables on what Julian day every visible new moon falls in any month of any Julian year past, present, or future.*

HAVING in the preceding Section accomplished the *primary* object of this *dissertation*, by explaining the most necessary articles toward a clear comprehension of the method and the several parts of these tables of the period of Calippus *during the first 76 years* after its commencement; I now proceed to the *secondary* object, which is, to point out how these Tables

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may be applied in an easy and popular method to discover the Julian Day of any visible new moon in *any other* period of 76 years, either before or after that first, and in any age whatever.

To find any visible new moon *after the commencement* of the Calippic period in the 330th year before the vulgar æra of Christ untill the introduction of the Gregorian Calendar in 1582.

From 330 substract the year *before* Christ next less than that year, in which we want to discover the new moon required; divide the remainder after subtraction by 76: what the quotient amounts to is of no moment, but the *remainder* after division will always express the year of the Calippic period then current, and which began at the autumnal equinox, *in* the Julian year wherein we want to find any new moon; the current Calippic year being so found, turn to in the tables, and there find the Julian month required. But, as an example is easier than directions,

Suppose, that I wish to know on what Julian day an eclipse of the Sun happened in Asia, which is mentioned by Historians as having been some time in August in the 198th year before Christ. In order to discover the day—From 330 we must substract 197, the year next *less* than 198, the remainder is 133, which divided by 76 leave 57 for remainder. This shews that the 57th year of some Calippic period was then current, i. e. had *begun* at the autumnal equinox *in* the year 198. On turning  
to



to the 57th Calippic year as marked in the 3d table, we must look for the month *August*; which if it had been a month *later* than the autumn we must have looked for *after* the N° 57: But *August* being *sooner* than the autumn, we must therefore look backward or *before* the N° 57. On finding *August* there, we find also *August* 8 marked as the day of the visible new moon; consequently the eclipse could have been only 1 day sooner. Accordingly August the 7th is the very day, on which modern astronomers have calculated, that the solar eclipse must have happened; a calculation, which would have taken the best astronomer some hours to investigate in a scientific way from Lunar Tables; but which is thus found in an easy and popular manner in two or three minutes by these Calippic Tables.—Observe here that the *first correction* necessary to be made in the method of Calippus, is, that, whereas his Greek months begin only at Sunset in the several Julian days expressed in my Tables, and therefore the *following* Julian day is his first Greek day; on the contrary, the *very day* expressed in my tables must be taken as the real day of each new moon: or in other words, Calippus made the Day next *after* the visible new moon to be the first day of the Greek months, we must therefore take the *preceding* day for the day *in which* the visible new moon happened.

In case the year in which we want to find any new moon be *later* than the commencement of the æra of Christ, the only difference caused by this is, that, instead of *subtracting* as above,

we must now *add* the given year of Christ to the 330 before that æra.—As for example, in the 30th year of Christ there was a central and total eclipse of the sun in Asia, but visible only to the eastward of Jerusalem at Sunrise; it happened in November, and if any one wishes to be ascertained concerning its precise day in that month, he must find the new moon in November by the method above described; that is, to 330 must be added the 30 years of the æra of Christ, the sum 360 being divided by 76 leaves 56 for remainder, which expresses the year of the Calippic period which *began* to be current in the 30th of Christ. On turning to N° 56 in the third table and looking forward for November we find *Nov. 15* marked as the day of the visible new moon in that year.

But here it becomes necessary to explain and apply a *second correction* requisite to render these tables accurate. For as 4 of Meto's periods of 19 years, i. e. 76, were found to be *too long* for the several moons in that number of years by 1 day; which made it requisite for Calippus to correct Meto by cutting off or omitting that excess of 1 day at the end of every period of 76 years: so the same happened to Calippus likewise; for Hipparchus discovered afterwards that 4 of the periods [of Calippus] would be still *too long* by 1 day; so that in every 304 years a little more or less it would be again necessary to cutt off 1 day in order to correct the period of 76. Now as the above sum of 360 exceeds 304, therefore we must cutt off or subtract 1 day from Nov. 15, as found above; and

and in like manner from every other date marked in the Tables, when the sum of years amounts to 304 or thereabout: the same likewise for every multiple of 304 years, be they ever so many. Thus then *Nov. 14* is found to be the day of the new moon, when *thus corrected*; accordingly this was the very day, on which the above total eclipse happened, as calculated by M. Pingrè\*. In this example the true new moon and the visible new moon (18 hours later) would both of them fall on the same day *Nov. 14* or before midnight. But whenever the visible new moon falls *earlier* in the day marked in the tables, then the true new moon may fall in the *preceding* night or afternoon at earliest: but where the *true* new moon falls is of little importance in history or chronology and only in astronomy; because it was the *visible* new moon, which the antients chiefly attended to in order to fix the commencement of the lunar months. Again, whenever the visible new moon notwithstanding this correction falls *later* than the day actually marked in the Tables, yet it will scarcely ever be so much later, but that it will be visible before sunrise in the *following* day: in which case that *following* day was still *often* considered by the antients as the first day of their lunar month; as is evident by the Macedonian dates in Ptolemy, as well as by some of the Athenian ones

\* "Ann. 30. Eclipse du Sol: 14 Nov. à 1 et d. m. As. cent. T." that is, "Sun eclipsed the 30th of Chr. the 14th of November at 1 and  $\frac{1}{2}$  in the morning, visible in Asia, central and total." Catalogue of eclipses in *Art de verifier les dates*, p. 51. Paris. 1770.



likewise; the same was also the practice of the Jews. So that it may be observed of these Tables, that whenever the day marked there does not, through the inequality of the Moon's motion, coincide with the *true* new moon, yet it will almost always be an inaccuracy on the best side, that is, the true new moon will have happened a little *sooner* (not later) tho scarcely ever in Greece and Palestine sooner than the afternoon of the preceding day, whereby it would still be not *visible* before the evening of the day marked in the Tables.—Hence Readers may form the following *general Rule*, that the *first* day of every lunar month among the antient Greeks would *in general* commence *at sunset* of the Julian day mentioned in the Tables, but in *some few* cases it might be 1 day later, or else 1 day sooner, in which latter case the day marked in the Tables would be their *first* day. This is sufficiently accurate for all historic purposes, tho not for those of astronomy; and indeed more accurate, than in any other mode of ascertaining the new moons by mean motion: neither would greater accuracy be of any use in History, because a similar latitude of inaccuracy actually prevailed *in the practice* both of those who employed a scientific cycle, and those who went by the visible moons; greater accuracy would therefore be only an attempt to determine by *certain rule* what was actually *uncertain in practice*. In referring then to solar eclipses I have in view only to try the accuracy of my Tables, by being enabled to ascertain more accurately the days of true new moons under all the improvements

provements made by the moderns in such lunar calculations. Let us therefore subjoin the two following farther examples.

In the year of Christ 29 M. Pingrè has calculated, that another total eclipse of the Sun happened in November, which was visible at Jerusalem, and the greatest obscuration about 12 o'clock at noon, the 6th hour of the ancients; it extended through Europe, Asia, and Africa. Now in order to ascertain the day of the *visible* new moon at this eclipse, To 330 we must add the 29 years after Christ, and the sum 359 being divided by 76, will leave 55 for a remainder; on turning to N<sup>o</sup> 55 in the Tables and looking forward for November, we find Nov. 26 marked as the day of the visible new moon: but 1 day must be here deducted as before, on account of the 304 years, and thus the visible new moon is placed on Nov. 25, which accordingly was the real day. For according to M. Pingrè the true new moon and solar eclipse happened on the 24th day in the meridian of Paris, at half after 9 in the morning; therefore it would fall above 2 hours later at Jerusalem, that is, near 12 o'clock on the 24th, consequently the new moon could not be visible that evening nor until near sunrise the next morning on the 25th\*.

Again, in the 33d year of Christ M. Pingrè has calculated another great solar eclipse

\* "Ann. 29. Eclips. du Sol: 24 Nov. à 9 et d. M. Eur, Asie et Afric. centr. 1." i. e. In 29th year of Christ, the Sun was eclipsed on November 24, at half after 9 in the morning, visible in Europe, Asia, and Africa, central and total.

in September, which was central and annular, therefore not so total as the former, but visible likewise throughout Europe, Asia, and Africa, and at Jerusalem in particular; yet near an hour later than the former, that is, the greatest obscuration would be at near one o'clock, but it would begin to darken about 12. Now in order to ascertain the day of the visible new moon by the Tables, if to 330 we add 33 and divide the sum 363 by 76, the remainder will be 59; on looking for N° 59 in the Tables we find *Sept. 14* for the day of the new moon: but 1 day must be deducted as before for the 304 years, which leaves *Sept. 13* as the right day; and this accordingly was the right day; for the true new moon happened according to M. Pingrè at the meridian of Paris on the 12th at half after 10 in the morning, therefore at Jerusalem about 1 o'clock \*; the new moon therefore could not be visible until after sunrise on the 13th, the very day pointed out by my Tables; so that at sunset on the 13th the first day of the lunar months of the Greeks and Jews would begin.

One or other of the two above mentioned eclipses was doubtless that of which Phlegon made mention in his annals; more probably however the first of them in 29, both because it was the greatest of the two, and because it

\* "Ann. 33 Eclips. du Sol: 12 Sept. à 10 et d. M. Eur. Afr. Asie, cent. A." In 33d of Christ Sun eclipsed 12th of Sept. at 10 and  $\frac{1}{2}$  in the morning; visible in Europe, Africa and Asia, central and annular. See a calculation of these 2 eclipses in Whiston's *fix dissertations*, p. 136 and 143.



also happened more exactly at the hour mentioned by Phlegon, namely, the 6th hour of the antients; it answered likewise best to the Olympic date attributed to Phlegon's eclipse according to the words of Africanus: hence it nearly agreed with the date at which Africanus and all other Christians in the first 3 centuries placed the Passion of Christ, viz. in the year 29; which circumstances have indeed been already observed and contended for by Kepler, Pagi, and Dr. Sykes \*. Eusebius places the eclipse of Phlegon 2 years later; but then he placed also the death of Christ 2 or 3 years later than Africanus, viz. in 31 or 32: and the 4th year of 202d Ol. began according to the Roman mode in January 32, but by the Greek mode not untill midsummer 32 after the passion of Christ. Therefore the Date of Phle-

\* The words of Africanus are "Luna toto orbe radiante [*εν πανσεληνω*] ab hora sexta ad 9 usque integram solis eclipsim, Tiberio regnante, contigisse narrat Phlegon." *Ap. Syncell. p. 322.* He gives no other date *here*, but *afterwards* he places the passion of Christ twice on the *second* year (*δευτερον* once at length) of the 202d Ol. which year did not begin even in the Roman mode before January in the year 30, a little after the eclipse in 29. But then Pagi has shewn, that Africanus (in which he is followed by Syncellus) places all historic events 1 or 2 years later by Olympic dates than other antients; the reason of which is, as may be seen in Syncellus, that he places the first Olympiad 2 years earlier before Christ than all others; so that his date agrees accurately enough with the passion of Christ in 29, if the above error be corrected. See Pagis *Critic. Annal. Baroni apud ann. Chr. 32.*

gon's

gon's eclipse in Eusebius does not agree any thing better with *his own* date of Christ's passion, than Africanus's does; for that 4th Olympic year did not begin until midsummer in 32 according to the Greek mode: it is also by the Greek mode only that it would still be the 4th Olympic year at the equinox in the following year 33, where the moderns place the passion of Christ; so that the date of Eusebius agrees best with the second of the above mentioned two eclipses, namely, that in September 33 \*. But the ancient Christians will not be found to have been so accurate in any of their chronologic dates, as that either the agreement or the disagreement with the passion or with the above 2 natural eclipses by either of the 2 dates attributed to Phlegon, whether that by Africanus or that by Eusebius (which ever was Phlegon's real date) can be esteemed a sufficient foundation for any conclusion either way; therefore Dr. Chapman might have spared his long dissertations for proving that the date 'in Eusebius did agree *scrupulously* with the passion of Christ in 33 †. There is moreover one circumstance,

\* The words of Eusebius are "Anno 4 Olym. 202 maximus, qui hominum memoria exstet, solis defectus visus est; nox erat hora sexta diei, ita ut astra in caelo cernerentur, sicut Phlegon hæc scribit totidem verbis [*ἐμφανὲς αὐτοῖς τὰ δει*]" *ap. Syncell. p. 325.*

† In his tracts *Phlegon examined, &c. against Dr. Sykes* 1734. I have entered into these particulars, because the subject has been lately revived by Mr. Rowyer in his *Conjectures on the New Testament*; and also abroad by M. Correvon in his notes to his translation of Addison *on the Christian Religion*. Tom. II. Geneva. 1772.

which

which all authors have omitted to notice, which is, that Thallus the *first* relater of this eclipse did in the confession of Africanus himself express himself in such words as convinced Africanus, that he *meant* a natural eclipse: there can be no pretence then to *suppose* that it was the supernatural one, let the date and words of Phlegon be what they may, who was only a copyist from Thallus long after the Event; whereas Thallus might have been a spectator of the eclipse, or at least have received his account from those, who were spectators of it\*. To this relation of Thallus Africanus opposes no other evidence than *his own opinion and supposition* that Thallus was mistaken: but the chief point in question, which it is so difficult to know, is, what these Heathen authors had *really said*; now the account of Thallus clearly deposes in favor of a natural eclipse, in the confession of Africanus himself, whether that of Phlegon was interpolated by Christians, or not, or misrepresented by himself, as if the darkness in question was at a full moon. Thallus indeed is placed by Vossius among the historians, whose age is doubtful: but it appears from this passage, that he lived after Christ; and as he is quoted by Justin Martyr in his *exhortatio ad Gentes*, he must have lived before the year 140 or thereabout. Justin seems to

\* Τοῦτο το σκότος ἐκλείψιν τοῦ ἡλίου Θαλλὸς ἀπικαλεῖ ἐν τρίτῃ τῶν ἱστοριῶν, ὡς ἐμοὶ δοκεῖ, ἀλογῶς. ap. Syncell. p. 322. *inter excerpt. Africani*.—"Tenebras has solis defectum vocat Thallus historiarum libro 3tio, *me quidem judice, absque ratione.*"

have



have never seen the work of Phlegon; for it was carried down to the 229th Olympiad, therefore to the year 137 if not later; but it might have ended several years before the time in which it was composed: and in fact we find that Phlegon was alive at a later period; for in his other work *de mirabilibus*, part of which is still extant, he mentions the consulship of Præfens and Rufinus in the year 153. If on the other hand Phlegon's work really preceded Thallus; which it is plain it could not do, except by an extraordinary nick in point of time: yet even in this case Thallus's account affords probable evidence, that Phlegon also had not mentioned this darkness otherwise than as a natural eclipse; and that the suggestion of its being at a full moon [*ἐν πανσεληνῳ*] must have been merely a supposition of the Christians themselves. This is farther confirmed by Origen's own words (if they be his) in his comment on Mathew, where he testifies, "that according to the assertion of the *Pagans*, Phlegon had never said that the darkness happened at a full moon" altho Origen himself has indeed contradicted this account in his tract against Celsus \*. But nothing is more common than

\* Latin. Tractat. in Matth. 35. The Pasch. Chronic. also quotes the passage of Phlegon twice, p. 219, 222, yet without any such words as *πανσεληνῳ* in it; and only observes immediately after, that Phlegon *knew the supernatural nature of this eclipse; it being never known that a natural eclipse happened at a full moon* [*γνους το παραδοχον της εκλειψεως*]; but no such knowledge appears in the words of Phlegon himself, as that it was supernatural or at a full moon, but only a very great eclipse.

for

for indiscrete zeal in Religion thus to pervert the judgements of men, and to injure the cause, which they meant to serve; this however is a defect, which Dr. Priestley cannot make against the Christian Fathers only, since he himself has given such conspicuous proofs of it in his own conduct: and in truth this is but too common a practice with all writers in modern times both orthodox and heterodox. For, however they pretend to be guided by philosophy or truth, yet they daily prove themselves to be slaves to their own prejudices in contradiction to the evident dictates of *rational criticism*: which is a guide to truth, that has in some degree preserved its dignity, consistency and character; while the decisions of Philosophy on one hand and of religious zeal on the other have both led to error, thro the vain pretences both of them make of being knowing and confident about divine matters, concerning which they often cannot possibly know any thing more than the most unphilosophic man of the multitude. The real truth in the present case seems to be, that when Christians about the time of Eusebius first began to conceive, that St. John had mentioned more than 2 Passovers, they then found it necessary to bring down the passion of Christ to a later year than 29; but they could find no later year, where they could fix it without great difficulties; which caused variations among themselves: along with the passion they likewise brought down its concomitant, Phlegon's eclipse, or else confounded it with the second eclipse in 33. In all these removals the error made by Africanus in placing all  
events

events 2 years *too late* in his Olympic dates would give them great assistance; until at last they produced such confusion, as we have too little evidence preserved to be able now to remove. This alone seems clear, that if the *general* error of Africanus in his Olympic dates *be* corrected; his present date of 2d of 202 ol. will agree sufficiently near either with the Passion in 29 or with the eclipse in Nov. 29: but if it *be not* corrected, then it will agree sufficiently with the year 30, 31 or even 32, in whichever of those years Eusebius placed the passion \*.

To

\* G. Vossius first revived the tradition of all the first centuries concerning the true year of Christ's passion being 29, in his tract *de temp. domin. passionis. Amst. 1643*. But he did not fix it on the right day. Pagi afterwards contended for the same year in *Crit. Baronii annal. ap. ann. 32. 1705*. but without attempting to reconcile this year with the circumstances at the Passion. He was however followed by *Ayrol de 70 Hebdomad. Romæ 1714*: and the same year has been lately adopted by Dr. Priestley in his *letters to the B. of Waterford* concerning the number of Passovers before the Passion, but still attended with the same deficiencies; whereby his opinion of there having been only 2 Passovers in all, is like a building without a foundation: for both He and the Bishop have reciprocally shewn, that there are such difficulties in the opinion of each, that nothing but the true year of the Passion ~~can~~ decide between them. I call therefore upon Dr. Priestley to make good his own opinion, and not again here also to *omit* that, which is of the most importance: it is a burden, which the support of his own otherwise tottering cause lays upon his



To find the Day of a visible new moon *after* the commencement of the new Gregorian Calendar in October 1582.

This case differs a little from the former one, but is still easier; for nothing more is required here, than after finding, as before, the right Calippic year in the Tables, together with the month in question, to *add* 4 to the day of the month marked in the Tables. For example, if it be required to know the day of the

his own shoulders; especially considering the manner in which he has spoken of the established clergy in his *Letters on the Dissenting Interest*, p. 28. We shall be glad to find any real improvement of the common Christian cause to come from any class of men. But if Dr. Priestley is too much involved in religious party and theologic romance to engage in any thing of such real utility, or if he can find nothing to offer to the public on this head, it may force me to lend him my assistance; as there is still something new to be said on the subject in favour of 29, which has not been urged by any writer, and in which these *Calippic tables* will be of use. But I would rather receive satisfactory information from others, than advance what may be thought less so: At present however I can find nothing satisfactory in regard to the year 33, where it is the modern fashion to place the Passion. For I cannot conceive that the first Christians for 3 centuries could be so greatly mistaken as between 29 and 33, beside other objections to this date: And that even their successors and pretended correctors should be still mistaken viz. Eusebius, Epiphanius &c, none of whom brought down the Passion later than 32. No person had ever mentioned so late a date as 33 before Roger Bacon.

visible

# 48 A P P E N D I X

visible new moon in January, February, March, &c of this present year 1788, we must add this number of years *after* Christ to the 330 years *before* Christ, which amount to 2118 years: now these divided by 76 leave 66 for remainder; so that the present year 1788 is the 66th year of a Calippic period. Turn therefore to the N° 66 in the 4th Table, and on looking backward from N° 66 to the January before it, we find *Jan. 4* marked as the day of the new moon, add 4 to it, they make 8; accordingly the new moon in this year was on the 8th of January at noon. To *Feb. 3* in the table add 4, they make 7; and the *true* new moon was actually on the 7th at 7 in the morning. To *Mar. 3* in the table add 4, they make 7; and the new moon fell on the 7th at night. To *April 2* add 4; and the new moon actually falls on the 6th at 1 in the afternoon. To *May 1* add 4; and the new moon does accordingly fall on the 5th at night. To *May 31* add 4; the new moon here again does really fall on June 4 at 9 in the morning. To *June 29* in the table add 4, they bring us to July 3; and the new moon falls on the 3d in the afternoon. To *July 29* add 4, they bring us to August 2; and the new moon falls on Aug 1. near midnight, therefore in the following day will be the visible new moon; and in almost all the foregoing cases the first day of the visible new moon would commence at sunset on the day found by the Tables. In the same manner we shall find all the remaining moons in this year to agree to the tables with sufficient accuracy for historic and popular use, and  
also

also with as much accuracy as ever did subsist in any Greek scientific Cycle, or even in any mode by visible observation; because a cloudy sky would often prevent observation, and oblige it to be supplied by conjecture or else to be often varied a day thro' other causes and motives. These examples then at the distance of above 2000 years *later* sufficiently prove the *accuracy* of these Tables, and consequently intitle us to depend equally upon them at the distance of 2000 or more years *before* the Calippic period commenced: They at the same time contain the best proof we can have, that I have understood the words of Geminus rightly, because they are found to tend to accuracy, whereas the tables of former chronologers contain nothing but error and confusion; and hereby we find moreover, that the retrograde course I have pursued, both pays us at last for the trouble of going backwards, and also secures to us a solid foundation on which we may build speculations concerning the more early ages of the two Assyrian empires.

It must be observed however, that the number 4 thus added will not always remain the same hereafter, neither are any more than 3 days to be *added in general* from the year 1582 to the omission of the intercalary day in 1700; after which it increases to 4, and it will be hereafter continually *increasing*; but whether at the rate of 1 day in every 200 years or otherwise, is an accuracy foreign to the present subject, and which I leave to the decision of others. Experience has however shewn us, that if the deduction of 1 day in 304 years be not altogether accurate,

E yet



yet that the error is so small as has produced no *sensible* inaccuracy in 2000 years during the currency of the old Julian Calendar. For the reason of adding 4 is, because there are nearly 7 periods of 304 in 2118; so that 7 must be *subtracted* from each date in the Table: but then the Gregorian Calendar by omitting 11 days has advanced each Julian day full 11 days forward; which must be afterwards *added* to each day in the Table; it is therefore the same thing, whether we first *subtract* 7 and then *add* 11, or add at first the difference between 7 and 11, which is 4. After the next omission of the intercalary day in 1800, the difference will then in a few cases begin to amount to 5.

To find the day of any visible new moon in any year *before* the commencement of the Calippic period in the 330th year before Christ.

There still remains this other case, viz. Whenever the new moon required *precedes* the Calippic period: now the chief variation, which this produces is, that here we must *add* 1 day in every 304 years instead of *subtracting* 1 day as in the foregoing cases *after* the Calippic period. For example, suppose we wish to be ascertained concerning the day of the new moon in May in the 585th year before Christ, when a celebrated eclipse has been noticed in History; which has been said (tho erroneously) to be the same eclipse, as that which is mentioned by Herodotus to have terminated the Lydian war

war between Cyaxares and Alyattes. In order to know the right day in May, we must in this Case *deduct* 330 from 585, the *remainder* will be 255; which sum being divided as before by 76 leaves 27 remaining; but these 27 years we must in the present case first subtract from 76, and then what remains, i. e. 49, will express the year of the Calippic period (which *began* to be current in 585) after we have added 1 to this remaining sum, which makes in the whole 50: and in the present case 1 must be always added in order to include the first year of the subtrahend, which is otherwise *excluded*. So that 50 was the current Calippic year in 585. Now on turning to N° 50 in the 3d table, and looking backward to May, we find May 28 for the day of the new moon; and so in fact it was: accordingly that eclipse, as calculated by astronomers, happened about sunset on May 28.—But then we must moreover observe here, that the dividend 255 approaches very considerably toward 304; now altho it may not be large enough to entitle us *certainly* to add 1 day, and thus increase May 28 to 29: yet thus far at least we may conclude, that when the *remainder* is so very large, if the *true* new moon did not fall 1 day *later* than in the Tables, viz. on May 29; yet it must have fallen *so late* in the foregoing day May 28 (as accordingly it did, namely about sunset) that the new moon could not *either way* be visible before the 29th; so that at least in order to determine the *visible* new moon in such cases we must always add 1 day, whenever the

*remainder* after dividing by 304 contains so large a portion of 304 as 255, or any such sum *before* the first Calippic Period. And so in like manner *after* the commencement of the Calippic Period we must *not subtract* 1 day (although the *dividend* amounts to such a very large portion of 304) if we wish to ascertain the *visible* new moon; because the true new moon must in this case also have fallen *so late* in the day marked in the Tables, that it could not be visible before the *following* day.

Again, as another example, there was a considerable solar eclipse on March 5 in the year 618 before Christ, which we shall find to have been the real solar eclipse, that is mentioned by Herodotus relative to the Lydian war; and after which eclipse so many erroneous calculations and enquiries have been hitherto made, altho in vain; the calculation of which eclipse by Mr. Hancock is annexed in this *appendix*: this is the very year, which I had pointed out in my Chronologic System at p. 217 of vol. 2. as being that in which the Lydian war *must have* ended; to this I was led solely by historic proofs in Herodotus and others, and in which year it *now* appears, that there actually was a considerable solar eclipse visible at evening to the eastward of Sardis, where the battle between Cyaxares and Alyattes must have happened. Thus when authors proceed upon solid principles for their foundation, one truth follows another; and those which were scarcely expected at first, yet at last will rise up to view, and justify their former reasonings and conclusions! But the misfortune is, that



too many proceed in the contrary way, by building up imaginary superstructures at first to amuse their readers with some agreeable systematic visions, and then afterwards search for a foundation for them: which if they are not able to find, they strain every thing in their favor, pervert and perplex all history, Chronology, Syntax, and Grammar, as Dr. Priestley has done; otherwise down fall Romance and their castles in the air.

Now in order to ascertain by the Tables the abovementioned eclipse in 618 before Christ, the 330 years of the commencement of the Calippic period before Christ must be first subtracted from 618, which leaves 288 for a dividend; and this sum divided by 76 leaves 60 remainder; which being subtracted from 76 leaves 16, and to this sum 1 year being added in order to *include* the year in question (which before was *excluded*) it follows that the 17th year of a Calippic period began in the 618th before Christ. If then we turn to the 17th year in the Tables, and look backward from N° 17 to the preceding March, we find *March 5* marked as the day of the new moon; accordingly that was the very day, on which the solar eclipse really happened. But as the dividend 288 contains a very large part of 304, this circumstance would have rendered it doubtful, whether the *true* new moon might happen in the evening of the 5th or on the morning of the following day, as mentioned in the foregoing case: yet even altho the true new moon did fall on the 5th, it would still fall *so late* in that day (as in the present case it actually did

a little before sunset) that the *visible* new moon could not appear before the following day the 6th, just as we concluded in the foregoing case.

If our chronologic enquiries should at any time lead us up to still more antient ages, yet the accuracy of these Tables will still support us in our investigations; and discover that in the 867th year before Christ (the 120th year before the epoch of Nabonassar) there happened a grand synchronism of the new moon and Sun on the 28th of March, the *reputed* day of the vernal equinox at the beginning of *Aries*, and in the *first* year of a Chaldæan great period, or *magnus annus* of 1460 intercalated years: such a synchronism could not have happened before, and could not possibly happen again under *all* the same circumstances in a less time than the vast period of 473040 years. An account of such a vast period among the Chaldæans has been actually preserved both by Cicero and Diodorus; but they have indeed related it (as in all probability the Chaldæans ostentatiously related it themselves) as being the period of their actually past *astronomic observations*: but it doubtless was a period, to the knowledge of which they had arrived only by *calculations* backward and forward from the abovementioned synchronism in the 120th year before Nabonassar; as I shall have occasion to shew more fully in my *Researches* concerning the duration of the Assyrian empire. It was at the above synchronism, that the Chaldæans began a new period of 1461 retrograde years (equal to 1460 intercalated years) and this was their *astronomic* period then current,

current, when all farther computation by it was superseded by the rise of the *historic* æra of Nabonassar in the 121st year of that period; as appears from the day on which the latter commenced, viz. the 26th of February: for in those 120 preceding years the Chaldæan new year's day would have necessarily gone back exactly 30 days, that is, 1 whole Chaldæan month from March 28 exclusively to Feb. 26 inclusively; with which day it was actually found to have been coincident in the 1st year of Nabonassar\*. Now if any person would wish, without the abstruseness of astronomic calculations, to convince themselves so far concerning these truths, as to obtain sufficient reason to be satisfied in a popular manner, that a new moon was actually coincident in that 867th year with the 28th of March, and the 1st day of that Chaldæan period of 1461 years, which then began at the equinox; he may in a few minutes convince himself of this very antient fact by the same process as the last. For from 867 he must deduct 330 as in the last example, which will leave 537 for a dividend; now this sum divided by 76 will leave 5 for a re-

\* Dr. Kennedy having maintained, that it began on Feb. 28, I expressed myself with some hesitation concerning this at p. 125 of Vol. I.; but having since examined the proof, which Petavius gives of this in *Doctrin. Temp. lib. 9. c. 51*. I am convinced of its truth: Petavius however, beside some little errors of the press in his 1st Edit. has expressed himself in some parts of his proof in such an abstruse and ambiguous manner, as might reasonably at first sight raise some doubts.



mainder : these 5 must here (the same as in the foregoing example) be first deducted from 76, the remainder will be 71 ; to which must be added 1 as before, which make 72, and these will express the year of the Calippic period, which *began* in 867. On turning to N<sup>o</sup> 72 in my Tables, and looking backward to March, we find *March 27* marked as the day of the new moon in that month of 867 : but then as the dividend 537 contains 1 period of 304 and 233 years more, we must here apply the *correction* by Hipparchus, and *add 1* day for the 304 years, which brings us to *March 28* for the day of the new moon ; and so it really was, if its date be computed by *mean* time, by which the Chaldæans probably did compute. But then, on account of the large remainder 233, the *visible* new moon must be expected still 1 day later, viz. on the 29th. We must remember also, that when the remainder contains so large a portion of a second period of 304, it is possible that the *true* new moon may likewise fall on the 29th : but then it will happen generally on the morning of that day, if it did not fall late in the afternoon of the preceding day the 28th ; in both which cases the *visible* new moon will still be equally on the 29th ; and our conviction of this is sufficient for all historic and chronologic uses. Mr. Hancock calculates, that in the present case the *true* new moon did fall very late, viz. about 10 o'clock on the 29th ; which if ever so accurate, yet it is probable, that the Chaldæans were not sufficiently acquainted with the Newtonian causes of the inequalities

equalities in the moon's motion to be able to ascertain so accurately by calculation, when the *true* new moon would fall: they probably *computed* by *mean* time when there *would be* a *true* new moon, and also *observed* when there *actually was* a *visible* new moon; which as it would appear before the morning that followed the 29th, they would in course place the true new moon 1 day sooner, namely on the 28th; and the same also would apparently be their conclusion, in case they went by the *mean* motion of the moon, as in a popular way will appear by a total solar eclipse in that very year calculated by M. Pingré. According to his computation, made under all the modern improvements, this eclipse happened in the meridian of Paris at a  $\frac{1}{4}$  after 9 at night on July 24 \*; therefore at Babylon 2 hours and  $\frac{3}{4}$  later, viz. about 12: Now if we multiply the moon's mean time 29. 12. 44' by the 4 moons between March 28 and July 24, we get 118 days 2 hours and 56 minutes for all the 4 moons; and these 118 days reckoned *backward* from Midnight after July 24, end at

\* “Ann. 866 eclipse du Sol: 24. Juillet à 9 $\frac{1}{4}$  soir, Asie, centr.—Ann. 867 eclips. du sol: 4 Août à 4 $\frac{1}{4}$  matin, Asie, centr.” *Hist. Acad. Bell. Lettr. Paris.* 1786, p. 100. In this work M. Pingré has calculated all the eclipses of the Sun and Moon for 1000 years before Christ. But observe, that he reckons the first year before Christ as 0; therefore his dates always contain a unit *less* than in the common mode; so that his 866 is the 867 of others.

midnight

midnight after March 28; so that thus the mean new moon would fall 2 hours 56 minutes before midnight on the 28th of March. M. Pingré moreover has calculated another solar eclipse on August 4 at a  $\frac{1}{4}$  after 4 in the morning in the meridian of Paris in the preceding year 868: now if we compute *forward* the 8 moons from thence to March 28 in the same popular way as before, which is accurate enough for my present purpose, by allowing 29, 12, 44' to each moon, the mean new moon would fall much sooner than before on the 28th of March 867, viz. at 10 o'clock in the morning. Thus then I have sufficiently explained the *method* of using these Calippic Tables for the purpose of a perpetual Almanac of the new moons: and at the same time I have verified their accuracy for all popular uses in history, by comparing them with the most accurate astronomic calculations of solar eclipses and new moons during the long period of above 2600 years, from that synchronism in 867 down to the new moons in the present year 1788; and we have found them to correspond in every case with truth.

As to the Chaldæans placing the equinox on the 28th of March, when it really fell in the year 867 before Christ either in the evening of the 29th or the morning of the 30th, it must be remembered that the antients always allowed 3 days for the equinox; on account of the difficulty in determining its precise time: and Hipparchus long afterwards, altho' he had invented a new and better instrument for that observation,



observation, yet erred to the amount of  $\frac{1}{4}$  a day, as M. De la Lande maintains \*.

In fact it was these Tables, which led me at first to notice that synchronism of the new moon and equinox on the 28th of March, and the important consequences which necessarily follow from it; such as that on this very day the Chaldæans must have begun either *their very first* or a *new* period of 1461 years; which period altho' well known to have been in frequent use or great celebrity in Asia, yet no person has been able, or has even attempted, to ascertain in what year any such period had been ever made to commence by the Asiatics or to end. The Egyptians indeed took upon them to determine the *Season* of the year, when any such period of 1461 years must have commenced, at least among themselves, namely when the Dog-star rises heliacally, which happens every year about the middle of July; but they never attempted (so far as any testimony remains) to fix on any *particular year* in their past History, in which they could affirm that any such period had ever commenced; except by *supposing*, that their current canicular period, as they affected to call it, had begun, when their new year's day had *formerly* coincided with that day in July on which the Dog star rises: nevertheless in their own *practice* they contradicted this *supposition*, and proved

\* " Ces equinoxes d'Hipparque, que l'auteur estimait exacts à un quart de jour près, ne le sont véritablement, qu'à un demi jour près."

*Mem. sur la longueur de l'année solaire. Copenh.*  
1784, p. 13.

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it to be merely a *speculative* one; for they themselves computed their own years from the historic epoch of Nabonassar or of Philip, not from the *supposed* year above referred to; a proof that they were ignorant of any other epoch of computation, and were mere copyists of this period of 1461 from the Chaldeans. Neither has any modern Author attempted to fix on any such year of commencement for any period of 1461 years; nor consequently to offer any reason, how it came to pass, that the Chaldæan new year and historic æra of Nabonassar should have begun at such an unastronomical time (as Scaliger somewhere justly calls it) as the 26th day of the Julian February; and not at any one of the 4 cardinal points, nor at any other point of commencement which we may reasonably presume that an astronomer would fix on. The æra of Nabonassar, by it's having an historic epoch, may account for that particular *year*, in which it commenced, being unastronomical; yet the same will not account for the particular *season* of year at which it commenced. But the origin of this now appears obvious, namely, because the Chaldæan new year's day *necessarily* fell in that year, the 747th before Christ, on the 26th of February, in consequence of the Chaldæans having begun either their very *first* or at least a *new* period of their magnus annus of 1461 years on the 28th of March in the 120th year before the epoch of Nabonassar: and this was the most proper astronomic point in the whole Zodiac, at which such a period of 1461 years could commence, especially among the Chaldæans; who in their astrology (some  
accounts

accounts of which have been preserved) and therefore (as we may presume) in their astronomy likewise (which has perished) always considered Aries as the first sign of their Zodiac. From the Chaldæans probably it was, that this practice extended to the astronomy of the most remote and barbarous nations at the farthest extremities of Asia and Arabia; and to the Romans, Greeks, and even the Egyptians also, notwithstanding that they themselves began their own years at a different season from the vernal equinox: and moreover notwithstanding that the Egyptians pretended to compute their own period of 1461 from the Dogstar in July, yet in their astrology they followed the practice of the Chaldæans; this is another relic which serves as a proof of their being only copyists in astronomy from the Chaldæans. Still farther, beside these *astronomic* testimonies (arising from the above synchronism and from the circumstance of the particular day on which the epoch of Nabonassar was fixed) that a Chaldæan *magnus annus* began in the 120th before Nabonassar; the same may be also confirmed by *historic* evidence in Syncellus, Agathias, and Polyhistor, as I shall shew in its proper place; these curious historic relics of Chaldæan chronology and astronomy having been hitherto neglected and almost unnoticed.

Now, this discovery, of the Year in which the Chaldæan period began, and also of that vast duration of 473040 years which they attributed to their past astronomical observations, will at least lead to a conclusion, that they had  
actually



actually attained to a more accurate knowledge of astronomy, particularly in regard to the length of the solar year, than Hipparchus and Ptolomey knew of, or than the moderns are inclined to believe; and that they made it to be 365 days 5 hours and 48', 53"; which is only 1 second less than the most *general* modern determinations; and the very same with some of the most *accurate* ones; but M. de la Lande contends, that it really is still less, and not above 48', 48"\*. For the difference between 365, 5, 48', 53" and 365 days, 6 hours, the length of the Julian year, being multiplied by 1460; and this product again multiplied by such multiplier of it as is necessary to enable the equinox to have performed a complete revolution backward throughout the Julian year, and to return to the 28th of March again (which multiplier is 32.4) the number of years will be 47304; but then at the end of this period the synchronism of the new moon in the equinox on the 28th of March will not fall in the *first* year of a period of 1460, for 47304 will not divide by 1460 without remainder: therefore it was necessary for the Chaldæans to turn the decimal of 32.4 into units and multiply by 324, or what is the same thing to mul-

\* "J'ai trouvé par les observations de M. de la Caille 48'. 49'', mais celles de M. Dagelet donne 50'', les observations d'Hipparque 48'', celles de Waltherus 50'', celles de Mayer 53'', de Tycho 46''. En s'en tenant à 48' on ne court pas risque de se tromper de 2''." *Longueur de l'année, Copenh.* 1784, p. 40.

tiply 47304 again by 10, before they could find such a similar synchronism as would fall in the *first year* of a period of 1460; and thus they produced their vast period of 473040 years; for this sum will divide by 1460 without any remainder, and it is the smallest period which will do so. This period of the Chaldæans is accordingly mentioned by Diodorus (lib. 2.) and he has only omitted the odd 40 years, just as when he mentioned the 520 years of the Assyrian empire in Herodotus (see my V. 2. 389.) he omitted the odd 20 years; reducing these periods in both cases to their whole numbers 473000, and 500. Cicero has omitted even the 3000 years, reducing the period for the same reason to 470000; for the odd 3000 years would appear to him of no moment in such a vast sum, to the truth of which he gave no credit. But fortunately Diodorus has preserved those 3000 years; which prove of this advantage to ourselves, that by the sum consisting of such broken numbers, and not of such whole and round sums as 600 or 36000 &c, we may conclude it to be impossible that such a vast sum should through mere accident alone happen to coincide with the *exact time* requisite to elapse, before such another synchronism could possibly happen under *all the same* circumstances as in the year 867 before Christ.

We may moreover conclude hence that M. de la Lande is mistaken, when he contends, that neither the period of 1461 nor a year intercalated like the Julian with 1 day in 4 years, was known either by the Greeks, Asiatics  
or

or Egyptians, before the time of Plato \*: for the knowledge of the difference between the Julian year and the true length of the solar year is the basis of the above computation by the Chaldæans of their still greater *magnus annus* of 473040 years. M. de la Lande should have recollected also, that the length of the Greek octennial period, which is exactly equal to 8 Julian years including the 2 intercalated days, as may be seen in Geminus †, is another proof that the length of the Julian year was known to the Greeks in their very earliest ages, the octennial period being of the highest antiquity there; and it was even superseded at Athens by the Metonic period before the birth of Plato: now as the Greeks borrowed their Astronomy from Asia or Egypt, the same knowledge must have prevailed there long before it did among the Greeks; and indeed this was apparently one of the first and most simple articles of astronomic science, to which antiquity had arrived in Asia. There is also some room for *presumption* at least, that the Greeks had originally preserved a memory of and a reference to that Asiatic epoch of computation in the 867th year before Christ by means of their Olympic games, which might have been not improbably first celebrated by Iphitus on the 4th year after that epoch or on some later multiple of that 4th year, both in commemora-

\* “Ainsi du tems meme de Platon on ne connaissait ni le quart de jour, ni la periode caniculaire [de 1461] quoique en disent plusieurs Sca-vans.” *Mem. sur la longueur de l’année astronm.* Copenhague, 1784, p. 9.

† c. 6.

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tion of the above Asiatic Epoch and also in order to make in the Greek solar year the intercalation of 1 day in every 4 years after the Chaldæan and Asiatic mode. The memory of those first Olympiads was afterwards so much lost, that the first Olympic year was in later ages and is at present reckoned only from the year 776 before Christ; this being the earliest Olympiad, concerning which any account had been preserved of the name of any victor at those games, who in that year was Coræbus: but it is allowed by all the antients, that the Olympic games had subsisted about a century before this, altho on account of the names of the victors being unknown (by the succession of whom the antients reckoned) they could not determine how long before 776 the games had been first instituted by Iphitus, a descendent of the Heraclidæ; who had not improbably brought along with them from Asia on their return to Greece many Articles of Asiatic knowledge. Now the year 776 was the 92d year after 867 inclusively; and it is generally conceived, that Iphitus had instituted the first Olympic games only a few years sooner, viz. in the 108th year before 776: there is no sufficient proof, however, that it was so early as to be before the Chaldæan epoch in 867; all certain evidence concerning those preceding Olympiads having perished. Syncellus does indeed inform us, that, according to one Aristodemus, Coræbus had been victor in the 28th Olympiad, which being the year 776, the 27 Olympiads before this are equal to the 108 years above-mentioned. But then immediately after he

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gives us another computation from Callimachus, who made only 13 Olympiads to have preceded that of Coræbus, which are equal to only 52 years; so much did the antients differ in their accounts of the date of the first Olympiad celebrated by Iphitus\*. The last computation is then just as probable as the first, while it suits our present object best: but as there are so many errors of date in Syncellus, it is possible, that 13 (xy) may be an error in the text for 23 (xy); these 23 Olympiads would be equal to the 92 years between the Chaldæan epoch in 867, and the time of Coræbus in 776. In this case Iphitus might have reckoned from the Epoch in 867 as the date of the first year of the Olympic æra, altho he did not celebrate the Olympic games untill some later multiple of 4 years whether that of 13 or more. But however this might have been, yet *at any rate* we learn from this account in Syncellus what a near coincidence there was between the Chaldæan Epoch and the original Olympic Epoch: yet whether the one was a copy from, or an imitation of, or had any reference whatever to the other, we cannot pretend to affirm; the following circumstances however give farther room to suspect some connexion between them in other respects beside an accidental coincidence; and that at the institution of those games an intercalated

\* "Coræbo victore Olympiade 28, ea prima deinceps habita est—De his narrationem contextit Aristodemus, et ei consentanea Polybius: Callimachus vero contendit Olympiades 13 omittas esse, et Coræbum victorem fuisse in 14<sup>a</sup>." p. 196.

year, like the Chaldæan and Julian, might have been first introduced into Greece by Iphitus, together with other rectifications of the Greek Calendar and mode of intercalating their lunar months by an octennial scientific period, instead of visible observations. Scaliger long ago suspected, that the Greek solar year had the intercalary day inserted at the Olympic games on every 4th year: Petavius indeed disproved some of the proofs, which Scaliger produced for his opinion, yet not altogether in a perfectly satisfactory manner. But the mere circumstance itself of a period of 4 years gives room to suspect some reference to the solar year; for such a period could have no connexion whatever with a lunar year; so that the fact may still be true, altho Scaliger did not or could not produce any sufficient proofs of the fact. In truth, it seems quite unaccountable, how the many different States in Greece could have ever agreed together with respect to the time and mode of intercalating the solar year with 1 day in 4 years, as we know they must have done (altho they never could agree together in any one thing else) unless it was done at some general congress of the whole body, as either at the Olympic Games or the Pythian ones: So little could they agree in regard to their lunar months, that, by their adopting such different modes or seasons of commencement, no two of their months seem to have ever coincided for 3 years together. Accordingly the words of the oracle, as given to the Elians, are very remarkable; when it was consulted by them, on ac-



count of the invitation sent them by Iphitus, to meet together and join in a common congress and league of the Greek States: they are preserved by Syncellus, of which the following line contains all the genuine marks of oracular ambiguity and abstruseness, Εὐτ' αὖ ἐνι τριόδοις ἐλθῇ φιλοφρων ἐνιαυτός \*. Now whether the lines in question were composed in later times or not is of no moment; if they had but been transmitted down from very high antiquity as the original answer of the oracle, they may in either case equally contain an intimation of there being some astronomical object in view relative to the Olympic games as well as a political one. If nothing more had been intended to be expressed by this line, than merely to recommend to the Elians to accede to a league for 4 years proposed by Iphitus; it is certainly a most strange, perplexed, ambiguous and truly oracular puzzle about a very simple question. The general ambiguity of such oracles, which commonly meant one thing, while they seemed to speak of another, gives us reason to suspect, that this line alluded to the regulations of the Greek solar and lunar years; which were made by Iphitus at the first celebration of the Olympic Games.—The jingle between ἐνι τριόδοις and ἐνιαυτός—the doubt whether ἐνι is the dative of εἰς or a poetic termination of the preposition ἐν—the common fanciful derivation of ἐνιαυτός from ἐνι and αὐτός, as if it meant that the year returned into itself every year—the opposi-

\* Syncellus, p 196.

tion between *εἰς* in the sense of *one* and *τριοδοίς* implying *three*, as if allusion was made to the usual intercalations of the lunar year *once* in *three* years—the opposition between this *triennial* intercalation of the lunar year and the supposed meaning of *εὐκαὶστος*, as if *every* year in course returned into itself—together with the unusual mode of signifying a period of 4 years by *three* being added to *one*; for the Greeks generally expressed a quaternial period by calling it a period of 5 years—all these conundrums give great room to suspect, that something more was understood to be agitated at the Olympic games, to which the Elians were invited, than a mere simple league of amity for four years. Several more circumstances might be added tending to confirm a connexion between the Olympic original Epoch and the Chaldæan astronomic Epoch in the year 867; and that the latter was apparently the great source and *primum mobile* of all the astronomic knowledge among the antients, which had been communicated from Chaldæa not only to Greece and Egypt, but to the utmost extremities of Asia. These facts would lead me too far from my subject; I shall however just briefly mention one, namely, that it appears probable from the circumstances attending the epoch in 867 and the commencement of the æra of Nabonassar in 747 on February 26, that the Chaldæans inserted their Intercalary day in their solar year two years *later* than was done in the Julian year by Sosigenes: now this will vindicate the present text in Censorinus (ch. 21) where the Chaldæan new-year's-day is mention'd

as being on the 12th of the calends of August; but which chronologers suppose to be an error for the 13th. For Censorinus would not improbably reckon up and fix in the Julian calendar the retrogradation of the Chaldæan new-year's-day, agreeably to the antient *order* and *time* of the insertion of an intercalary day in the solar year by the Chaldæans themselves (the knowledge of which might have been still in the age of Censorinus preserved among the Egyptians), and not according to the new mode introduced by Sosigenes into the Julian Calendar: just as we at present fix the retrogradation of the Chaldæan new year agreeably to the *right* mode of intercalating the Julian Calendar as originally established by Sosigenes; and not according to the *erroneous* method, which had afterwards subsisted at the beginning of it, until rectified by Augustus. It would not have been a less impropriety for Censorinus to have employed the Julian mode in preference to the Chaldæan mode, than for him or ourselves to have preferred the *erroneous* methods employed at first by the Roman priests under Augustus to the right mode of Sosigenes. But we indeed at present are forced to employ the Julian mode, because we have lost all knowledge of the Chaldæan method; this however ought to be no reason for editors to corrupt the dates in the text of Censorinus and other authors, in order to reduce them to the same erroneous method as we are forced at present to adopt ourselves, because we possess no other method: for it seems to follow from the epoch being in 867, that the Chaldæans must



must have employed a different order of intercalary years from the present Julian one \*. Why Sosigenes placed his own intercalary day 2 years sooner than the Chaldaëans we can not pretend to ascertain; but possibly he might conceive it to be more accurate, or he might have some reason peculiar to the Romans themselves: yet our ignorance of one fact ought

\* For the same reason the period of 20 years in Livy's account of Numa's calendar, as it is found in *all the MSS. of Livy*, ought to be preserved by Editors, instead of their own interpolation of 24 years: 20 is an aliquot part of 60, and it is said, that the Bramins in India reconcile their lunar and solar months at the end of every 60 years, altho we know not how. Both 20 and 60 are also aliquot parts of 120, at the end of which period the Persians intercalated a whole month of 30 days in their solar year, instead of the Julian method of 1 day in 4 years. It may be doubted also, whether by 20 Numa did not mean to signify a period of 19, in the same mode of expression as the Greeks called the Olympiads periods of 5 years: Thus the period of 19 might have been known in Italy long before Meto adopted it, just as it seems to have been long before known to the Indians and Chinese. Scaliger says of the Roman calendar, *Nulla gens in terris ineptiore anni formâ usa est*. Lib. ii. 187. This is certainly true, if meant of their calendar as it existed in *practice*. But still a doubt remains, whether Numa had not instituted a more excellent calendar in *theory*, if it had been adhered to; and whether it was not a lunar-solar one, not merely a solar one as afterwards in the Roman corrupted *practice* of it. Cicero at least expressly affirms *a Numa perite institutum est*, and several circumstances confirm this; 20 therefore ought not to be changed to 24, by conjecture.

to be no pretence for our corrupting the date of another fact; and we might just as well reduce the dates in English authors at which Easter formerly fell according to the old Nicæan method, to the dates at which it would have fallen according to the present Gregorian method. Many other hints and conclusions seem to follow from this discovery of the original epoch of Chaldæan computation in 867; which will apparently serve as a key to open the door to some of the lost *Arcana* in the dilapidated musæum of both Greek and Asiatic antiquities; as I may take a future opportunity to display still farther.

At present however we may conclude thus far with safety, that M. de la Lande is a much better astronomer than antiquary in regard to prophane customs and facts; just as Dr. Priestley is a much better experimental philosopher than historian in Christian antiquity; knowledge in one science implies no necessary knowledge in another. Accordingly in fact I have repeatedly found, and repeatedly proved also in these *Observations*, that while the moderns are daily censuring and detracting from antient authors, both Christian and prophane, imputing to them at one time error and negligence, and at another credulity and ignorance, pretending also that they correct, when in fact they are corrupting the text and meaning of those authors; yet at the very same moment they are daily exposing their own inability and rashness: for they allow themselves neither the necessary time nor trouble so much as even to understand those antient authors, whom they condemn;

demn; and whose text and meaning they too hastily judge of by the tenor of their own modern practices, errors and suppositions; thus they become at last evidences against themselves, and only prove and display to public view their own mistakes, inattention, and self-confidence \*.

\* I may here observe, that M. de la Lande has fallen into the same error as that of M. Costard, pointed out in *Vol. II. p. 43.* by his not attending to the retrograde nature of the æra of Nabonassar; for he likewise has placed the eclipse (mentioned by Theon) in the year 365 instead of 364: "Une eclipse fut observée par Theon à Alexandrie l'an 365." *Astronom. vol. I. p. 159.* Bullialdus had before made the same mistake in his *Astron. Philolaic. Par. 1645, p. 334.* Weidler the same in his *Histor. Astron. p. 189.* Vossius also *de scient. mathem. p. 165.* Vanderhagen likewise in *Observ. in Theonis Fastos. Amsterd. 1735, p. 34.* Thus we find, that even Astronomers have their hereditary errors, which they as blindly copy from each other as Historians, Unitarians, and Commentators; and they all stand equally in need of the sober corrections of Chronology; the mistake of a whole year in the date of an eclipse is much the same as that by Dr. Priestley in placing the *later Platonists* 200 years before the first of them was born. Chronology is indeed a Science, which extends its assistance to all others, and helps to moderate that extravagance of modern prejudice, which makes men rash enough to assert any thing. Thus Jackson, like M. de la Lande, affirms: "That the solar year of 365 days and  $\frac{1}{4}$  was not known to either Greeks or Romans, till Eudoxus brought it into Greece 300 years after Numa." *Vol. II. 63.*

Lastly,



Lastly, these Calippic tables have another peculiar advantage, above all other methods of computation, which is, by their presenting to our view at one and the same time a complete state of all the new moons both *sooner* and *later* in the same year, and also during many different years as well *before* as *after* any particular year under our immediate consideration. Nay still farther, these tables will at a glance present us with a view how far in any other cycles or scientific periods (such as 8, 19, 60, 84, 120, &c.) the days of new moons, as pointed out by them, differed from the real truth as ascertained by the more accurate Calippic period; and this not only during the course of any such single cycle, but likewise in any multiple of such cycle tho ever so many, either before or after any given date: for by tracing either *backward* or *forward* in these tables any particular given year of any such cycle, as for instance, from 8th year to 8th year, or from 84th year to 84th year, and so on, we may extend our enquiries to any length of time whatever. In order to this, if our enquiries respect the cycle of 84 (for example) since 84 exceeds 76 by 8, we have nothing more to do, than for every multiple of 84 to look 8 years *earlier* or else 8 years *later* in the tables; and thus to find in what Julian year any new cycle of 84 commences, and consequently how the new moons fell in any year required of any such cycle of 84, or of any other.

In regard to the *theoretic principles*, on which these tables are constructed, they have been preserved with sufficient perspicuity by Geminus  
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in his astronomy of the antients \* ; but this is a subject foreign to the purpose of the present *dissertation* ; which proposed to go no farther, than to explain the *method* of using these tables, and how to apply them to *practice*, and moreover to verify the *accuracy* of them by proper examples, as well as to give some specimens of the *assistance* which may be obtained hence toward the illustration of various historic, astronomic, critical and chronologic questions in both prophane and Christian antiquities ; by means of the easy, obvious, popular and expeditious manner in which these tables may be applied, to confirm and vindicate the integrity of the text, the truth of dates, and the propriety of many expressions in antient authors ; in case they have any connexion with or relation to the times of new moons, or full moons, or eclipses of either the sun or moon. Such antient facts may be hereby not only shewn to be demonstrably true, but also in some degree rendered ocularly perspicuous to us ; and thus conviction of the understanding in readers will be produced as it were by the operation of their own senses, instead of their depending altogether upon the accuracy of the writer, or the reputed credit of adepts in astronomic calculations : for after all, the latter method never affords any thing more than a silent acquiescence in the truth of what is read, together with a kind of dim and doubtful persuasion, far inferior to a full conviction of the mind. So that upon the whole,

\* In Uranologia Petavii, c. 6.

we shall hereby find more and more, that chronology does not terminate in mere speculation, but is a necessary servant in the study of antiquity to the critic as well as theologian, to the historian as well as antiquary, and even to astronomers likewise; and that it ought to be always kept ready at hand to perform the office of a touchstone, by assisting us to distinguish between truth and error, between real facts and fanciful suppositions, between correction and corruption, between actual knowledge and the quackery of pretended learning: the necessary consequence of false knowledge is, to damp and stop the progress of enquiry, and thus by means of favorite systems of error to establish the reign of doubt and darkness; whereby scepticism finds it an easier task to extinguish the divine lights to reason, which may be derived both from the scriptures and prophane authors. It may indeed be questioned whether if Christian and prophane authors were alive once more, they would know their own writings again; as they appear now in the hands of the moderns, under the several alterations, explanations and commentaries, which they have undergone: but certainly they could not fail often to laugh at the strange blunders of the moderns concerning their meaning, and as often be shocked at the extravagant absurdities, which they themselves have been made to say, but which their writings do not really contain; and the more so for being at the same time gravely complimented by some on account of the excellence of their pens; while on the contrary they are as much abused by  
others



others for their ignorance and errors. Whence does this arise, except from the too frequent practice of the moderns, in never looking at home to their own errors? if Mr. Gibbon or Dr. Priestley had done this, they would not have departed so often from truth in regard to the history of Christianity, nor have made such inequitable judgements concerning the first Christian fathers, who were in fact a most wonderful set of men; for thro' the strength of their Christian principles they in reality possessed that sublime philosophy, which the heathens only talked of and disputed about, but never attained to: neither to such firmness in their principles, and such perseverance in their conduct, could even the Christian fathers have attained, if they had considered Christ in the cold mode of a mere man or philosopher, and not as a divine instructor. So that it is to the doctrine of the divinity of Christ, that the successful establishment of Christianity is to be attributed, more than to most of those other secondary causes adduced by Mr. Gibbon: it was indeed a doctrine which necessarily became a spur to the inactivity and a damp to the selfishness of one part of mankind, as well as a means of reverence and admiration from the other. My own *observations* therefore have been invariably directed to moderate this overweening fondness of the moderns for themselves, by inducing them more frequently to look at home; and when they see what strange mistakes are daily made even by the learned with respect to antiquity, hereby to lead others, if possible, to a more just and impartial

tial judgement concerning the antients, both Christian and prophane. The sum then of the whole is, that we do not take sufficient pains to understand the writings of the antients, neither those of the Jewish scriptures, nor of prophane authors, nor yet even those of the Christian fathers, as I have repeatedly proved against Dr. Priestley in my *Appendix* to the 4th volume: so that one of the greatest novelties at which authors can aim during the present rage for innovation, is for such innovators to comprehend rightly themselves what it is they say and write. What has been the subject of my four volumes of *Critical Observations*, but to point out the hasty mistakes, the rash assertions and false reasonings to be met with among all classes of writers? and altho volumes should be yet added to volumes, they would be like the rolling snow-ball, which increases more and more in its progress. Can we then be said to have arrived at the highest pitch of knowledge in the present age, when one of the first things necessary to a learner, is to perceive and unlearn the errors, which subsist in the most capital books of his library; and these also such as often totally invert the right view of things, and teach him nothing but confusion?

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*A Discovery of the right year before Christ, viz. 618, in which that solar eclipse happened, which is mentioned by Herodotus as terminating the Lydian war between Cyaxares and Alyattes—together with a calculation of this eclipse, by Mr. Hancock Teacher of Mathematicks in Norwich, proving, that it happened on March 5, and that the greatest obscuration was at least 26 minutes before sunset at Sardis, and still more within 7 degrees eastward, to which distance the eclipse extended: hereby the errors of M. Pingré are corrected in his calculation of the same eclipse; who places it on March 6, and takes no notice of its being visible in Asia Minor, but only in Europe and Africa, in his catalogue of all eclipses for 1000 years before Christ, inserted in tom. 42 of the Hist. de l'Acad. Bell. Lettr. Paris, 1786. p. 114\*.*

**I**N my brief sketch to lay the foundation of a true System of antient Chronology in vol. 2 of *Critical Observations* p. 217, I placed the end of the Lydian war in the 618th year before the vulgar æra of Christ; which is 11 years sooner than in any other chronology: to this date I was led by historic proofs in Herodotus and other antients; and I there observed on this

\* “ Ann. av. I. C. 617. Eclip. de Sol: 6 Mars à 3 h  $\frac{1}{2}$  S. Eur. S. Afr. centr. A. i. e. In the 617th year before Christ the Sun was eclipsed on the 6th of March at  $\frac{1}{2}$  after 3 o'clock in the afternoon (in the meridian of Paris) visible in the South of Europe and in Africa, central, and annular. See above p. 57. in note.

subject



subject "that in this year must have been the great solar eclipse mentioned in Herodotus: If any one would calculate it, it would decide as to the right year: no one has hitherto sought for it sooner than the 605th year before Christ\*." I could not at that time hope to have predicted so very accurately as I now find that I did; altho' I had indeed then good reasons to conclude, that I was not far distant from the truth: for it *now* appears certain, that a considerable eclipse, altogether suitable to the relation of Herodotus, happened in that very year 618, and indeed more suitable in several circumstances than any other, which has been hitherto suggested by other writers. On my application afterwards to Mr. Hancock several years ago, he readily undertook to calculate, whether any such eclipse had really happened about the year 618, as I there pointed out; and he soon found, that there really was one quite suitable to my purpose in that very year 618, and no other for 4 or 5 years either before or after it, which could be the eclipse in question. It was visible 7 degrees to the North Eastward of Sardis; where the battle, as related by Herodotus, between Cyaxares and Alyattes must have happened: 7 degrees would extend beyond the bounds of the kingdom of Alyattes; for Herodotus expressly notices, that Cræsus long afterwards altho' he had

\* I should have said the 607th, for altho the date of the eclipse adopted by Calvisius is indeed 605; yet it must be observed, that he did not reckon back from the vulgar æra of Christ, but from the supposed year of the birth of Christ 2 years sooner.

conquered

conquered many nations in those parts, yet had not extended his dominions beyond the river Halys. This eclipse, by its greatest obscuration being a little before sunset, in the mild rays of the evening sun, would also have been more observable and a more awful spectacle to the two armies, than at mid-day. I intended to have published Mr. Hancock's discovery of this eclipse sooner, but was diverted from it by the intervention of Dr. Priestley's romance in place of this more agreeable pursuit after truth: and in the mean time M. Pingré has published his catalogue of all the eclipses before Christ, as mentioned above; among which he notices this, but not accurately, nor yet indeed with such circumstances as could have pointed it out to myself or to any other person, as being the eclipse referred to by Herodotus, if it had not been for the assistance and greater accuracy of Mr. Hancock.

However altho truth is thus come out at last, yet it may be proper just to take a little retrospect of the causes, which had so long detained former chronologers in error. Now some of the antients had erroneously mentioned a considerable solar eclipse on May 28 in the year 585, as being that referred to by Herodotus; and yet at the same time they had inconsistently mentioned, that it happened in the reign of Astyages, not of Cyaxares his predecessor; there was however some good pretext by which they were drawn into this error, altho it is foreign from our present subject: But modern chronologers soon perceived, that this could not be that eclipse mentioned as ter-

G

minating

minating the Lydian war; because Cyaxares could not possibly have been still on the throne so late as 585, nor later than 593 years before Christ. Hence Calvisius, Usher, and Petavius, sought for some considerable eclipse, which happened before 593. Petavius found one in 597; Usher another in 601; and Calvisius another in 607: yet none of these were greater than that in 618; they being all about 9 digits and  $\frac{1}{2}$ . But there is one common and of itself alone a sufficient objection to these other three eclipses, which has not however been noticed by any one of those chronologers, nor by any other person, notwithstanding that they have all reciprocally criticised each other; this is, that altho those eclipses do indeed all fall within the reign of Cyaxares, yet they fall too late in it to be consistent with the relation of Herodotus, the earliest of them being not more than 10 years before his death. By the account of this historian it is evident, that the eclipse, to which he alluded, must have happened nearer to the *beginning* of the 40 years reign of Cyaxares than to the *end* of it; and as Herodotus is the only antient, who mentions the circumstances attending the Lydian war and the eclipse in question, we can have no right to make arbitrary suppositions of our own, in contradiction to the tenor of his narration: more especially if we observe that the events, which he relates in the reign of Cyaxares, are connected together by such expressions, as clearly point out that the real succession of events was agreeable to that order, in which



which he has related them \*. These words *Totâ supra Halym Asiâ sibi conciliatâ*, and *collectis suis omnibus copiis duxit eas adversus Ninum*, shew plainly, that the Lydian war had *preceded* his *first expedition* against Niniveh; at which time it was, that the Scythians first entered Media, who continued there 22 years, before Cyaxares could expell them again: consequently the Lydian war must have ended *above 22 years* before the *end* of the reign of Cyaxares. But all our chronologers, by placing the eclipse and end of the Lydian war at not more than 10 years before the end of the reign of Cyaxares, have in consequence been forced totally to invert this succession of events in the subjoined narration of the historian; and to place both the invasion and subsequent expulsion of the Scythians *before* the Lydian war and eclipse. All this has been the effect of placing the eclipse so late in the reign in question, and for which there is no other evidence than merely the arbitrary conjectures of chronologers; in their several accommodations to which they have not scrupled to turn the account of Herodotus upside down. Omitting then all such previous conjectures about the year of the eclipse, I followed strictly in my chronologic system the narration of the historian in regard

\* "Cyaxares is est, qui cum Lydis bellum gessit, quo tempore in ipsâ pugnâ dies in noctem est conversus: quique totâ supra Halym fluvium Asiâ sibi conciliatâ, collectis suis omnibus copiis, duxit eas adversus Ninum; ipsi vero, Assyriis in prælio superatis, Ninum obsidenti ingruit ingens Scytharum exercitus." *Lib. 1.*

to the order of the events as related by him; conceiving this to be the right means of being conducted by him to discover the real year of the eclipse, or at worst to be not far from it: accordingly we now find that this method has led us safely to the very year in question, and with more accuracy than I could have expected; whereby it has decided what was altogether precarious and undetermined before. For those 22 years *before* the year 593 carry us up to 615; but as Cyaxares, after expelling the Scythians, conquered both Persia and Niniveh, we must allow a year or two for these two expeditions after the end of the above 22 years of the Scythian invasion: therefore the Lydian war could not have ended *later* than the year 617; and for other reasons which may be adduced, it could not have ended *sooner* than 619; hence I concluded, that the eclipse in question and end of the Lydian war must have happened in one of those 3 years 619, 618, or 617. I took therefore the middle number, as presuming that thereby I should be the least distant from the truth; and hereby I hit accidentally upon the very year of that eclipse in question, which has been so much enquired after; and which indeed is of much importance, by fixing so many other dates of events connected with it\*. This earlier date of the  
end

\* It must be observed, that Usher, Petavius, and others, make the Scythian invasion to last 28 years; but the computation by Herodotus, which concludes his history of Astyages, proves, that it was only 22 years. Herodotus however does in another  
place,

end of the Lydian war is one of the chief articles, in which my chronologic system differs from that of others; a second article is in my placing the beginning and end of the reign of of Cræsus 5 or 6 years *earlier* also than others; and a third in my making Alyattes commence his reign about 8 years *earlier* likewise: all these dates are connected with and dependent upon each other; and the proofs of them all are chiefly drawn from Herodotus, and from such passages in him as have been hitherto overlooked. The whole of these dates in Herodotus are indeed consistent with each other; and at the same time his accounts as well as dates so consistent in general with the Jewish scriptures, that I cannot but consider such a coincidence to be a better testimony to truth, than any collection of independent events and disjointed systematic dates by the antient Christian chronologers, or than the conjectures of the moderns; we have had a remarkable example

place, namely, in his history of Cyaxares, seem at first to contradict his other account; and to make the invasion last 28 years: but on closer attention it appears, that he meant *here* only, that the Scythians had been *absent* 28 years *in all from home*; his word is ἀποδημασάντες; and they had not been all this time in Media. For Herodotus tells us, that they followed the Cimmerians out of Europe into Asia; and, instead of going the nearest way to Media, they took a *very long circuit* to the north of Mount Caucasus, and round the Caspian-sea, and thus entered Media on the *east side* of it: these long migrations would probably take up the *first* 6 years out of the 28 elapsed from the time of their leaving their original *residence* in Europe.



in the present article, how little the moderns scruple to turn every thing upside down in order to render their own imaginations plausible; along with Dr. Priestley they seem ever ready to extend the region of Romance either to Theology, or to Chronology, or to History, and even to Astronomy, rather than balk their inclination for the art of desperate conjecture.

I shall at present only add one farther corroborating proof from Eusebius, that the end of the Lydian war and eclipse fell on the year 618; because it will at the same time in some degree point out also the necessity of our making those other corrections concerning the *earlier* commencement of Alyattes and Cræsus. Now Eusebius, in his Chronicon, has preserved *two* dates of the capture of Niniveh by Cyaxares; one near the *end* of his reign; and another nearer to the *beginning*; and indeed not far distant from the right year of his reign, in which his *first* expedition against Niniveh happened. This double date of the same event has perplexed his editors. Pontacus however has thrown a little light upon it by just briefly observing, *Eusebius hoc loco videtur imitatus Herodotum*: yet it might be added farther, that he had not only followed Herodotus in making two expeditions of Cyaxares against Niniveh, in the first of which he failed, and in the second was successful; but that he had also fixed his date of the first expedition exactly in the *very year* pointed out by Herodotus, namely *immediately after* the above mentioned eclipse in the year 618. For at the 3d year of the  
40th

40th Olymp. it is, that Eusebius has those words, *Cyaxares adversus Assyrios dimicans, Ninum civitatem capit*: Cyaxares did not indeed at that time *take* Niniveh, being prevented by the irruption of the Scythians; but the last half of this 3d year of the 40th Olymp. coincides with the year 617, the very next year after the eclipse; to which date the narration of Herodotus agrees, and which I have accordingly adopted in my chronologic system as the date of the *first* expedition of Cyaxares. It is probable, that Eusebius copied this date out of some good system of antient chronology, and it contains a strong confirmation, that the eclipse in 618 was that referred to by Herodotus; since his own words prove, that it happened *immediately before* the expedition of Cyaxares against Niniveh. It is observable; moreover, that *both* the passages in Eusebius concerning Niniveh are inserted among the events of the Lydian kings, not the Median kings: this seems to indicate, that Eusebius extracted them from some Lydian History, and that originally they were connected with some relation concerning the Lydian war and Alyattes; this may account for the accuracy of the former of those two dates. Pontacus has made some little mistakes concerning these dates; and has without any good reason removed the words of Eusebius from the dates, where they are found in the oldest editions of the Chronicon, that of Stephens in 1512, and the Basil edit. in 1529. When will editors cease from corrupting the text of authors by their fanciful conjectures? Readers will obviously

perceive the important consequences of the Lydian war ending *so early* as 618, with respect to the accession of Alyattes; who must of necessity have been then above 5 years upon the throne, as he was a principal party in that war: therefore he must have reigned 57 years in all, as Herodotus says he did; and not 49 only as in Eusebius, who does not place him on the throne untill several years afterwards. In this error he has been followed by Petavius, and others; but the consequence of Alyattes acceding to the crown *earlier* is, that Cræsus must have acceded earlier also: which it may be equally proved from Herodotus, that he must have done; consequently Herodotus was under no error in making Solon converse with Cræsus. All those five events, the Lydian war, the eclipse, the accession of Alyattes, the accession and end of Cræsus's reign have been placed *too late* by chronologers; an error into which they seem to have been led by some systematic chronologers among the antients in times later than Herodotus, who had indiscretely taken the romance of Xenophon for true history: hence after having, like Xenophon, first *brought down* the capture of Cræsus to be coincident with the capture of Babylon, in consequence they brought down *too late also* all former events, connected with those kings; and thus they made them all inconsistent with the more antient and more accurate accounts of Herodotus, and of the Parian marbles, which generally agree together. Apollodorus, or some chronologers about his time, seem to have been the authors



of these anachronisms, which have misled our modern chronologers, and caused so many doubts, discordancies, and difficulties, in regard to prophane authors themselves, as well as in regard to the Jewish scriptures: Xenophon's Romance has been the ruin of chronology; and Dr. Priestley seems desirous to render Romance equally the ruin also of Theology, Antiquities, Criticism, and Christianity\*. He indeed unjustly styles me the modern *Pere Hardouin*; yet the directly contrary is the real fact; for I have been uniformly the defender of the originality, authenticity, and consistency, both of the Jewish scriptures and of Christian and prophane Historians against the *bevvues* of all the *Pere Hardouins*, the *Priestleys*, and other modern conjectural innovators of all denominations: and long possession of error by the moderns can give no prescriptive right in history and chronology, as it does in law; nor supply any good reason why they should not be ejected out of possession of those errors again, except that reason urged by the pleasant dreamer in Horace, *Pol me occidistis amici*; that is, because the moderns would rather continue in errors, which long possession have rendered agreeable, than to be awakened to open their eyes by an unwelcome stroke from the rod of truth.

\* See vol. I. p. 117: and vol. II. p. 193.

The following calculation of a Solar Eclipse which happened near Sardis in Asia-minor, in the afternoon of March the 5th, in the year 618 before Christ, was deduced from the *Tabulæ Dunelmenses*; the Moon's mean motion being accelerated according to the directions laid down by the Reverend Spencer Cowper late dean of Durham, viz. The correction of mean motion being  $2' 20''$  for each century from the Radical Year Minus before and Plus after 1720; and the acceleration is according to  $10''$  for each square century from the radical year Plus before and after 1720; whence by connecting those quantities the total correction of mean motion in any century may be rightly determined.

H A V I N G thus accelerated the Moon's mean motion, I find the mean time of Ecliptic $\delta$ in the Meridian of Greenwich happened on the 5th of March Ante Christum 618 P. M.		$2^h 18' 20''$
Which reduced to the Meridian of		
Sardis Long. $29^\circ$ E. or		$1 \quad 56 \quad 00$
Ecliptic $\delta$ mean time at Sardis		
Mar. 5 ante Christ.		$4 \quad 14 \quad 20$
Equation of apparent time Subt.		$0 \quad 14 \quad 6$
Apparent time of Ecliptic $\delta$ at Sardis		$4 \quad 00 \quad 14$
Sun and Moon in the Ecliptic then $\times$		$8^\circ 40' 25''$
Moon's Latitude at that time N. A.		$0 \quad 1 \quad 27$
Moon's Equatorial Horizontal Pa-		
rallax		$0 \quad 53 \quad 56$
Sun's Horizontal Parallax on his		
Altitude		$0 \quad 09 \quad 7$
Sun's Semi-diameter		$0 \quad 16 \quad 2$
Moon's Semi-diameter on her Altitude		$0 \quad 14 \quad 51$
Moon's Reduction from her Orbit		
to the Ecliptic		$0 \quad 0 \quad 4$
Sun's Right Ascension		$340^\circ 17' 52''$
Apparent time from Noon +		$34 \quad 35 \quad 00$
Right Ascension of the Medium		
Cæli		$374 \quad 52 \quad 52$
		Complement

# TO VOL. THREE. 91

Complement past $\gamma$	14	52	52
Medium Cœli in the Ecliptic $\gamma$	16	9	23
Meridian Angle	67	21	24
Declination of the Culminating point N.	}	6	21 50
Altitude of the Equator at Sardis			
Altitude of the Medium Cœli	51	50	00
Altitude of the Nonagesima Degree	58	11	50
Medium Cœli from the Nonagesima	60	54	00
Nonagesima Degree in	13	25	38
Moon from the Nonagesima	$\gamma$ 29	35	1
Horizontal Parallax $\text{D} \hat{a} \odot$	50	54	36
Parallax in Long. $\text{D} \hat{a} \odot$	0	53	49
Moon's visible Long. Occidental	0	36	30
Moon's true Lat. N. A.	$\times$ 8	3	55
Parallax in Lat. $\text{D} \hat{a} \odot$	0	1	27
Moon's Visible Lat.	0	26	49
	0	25	22

From the Nonagesima Subt. 3 Signs the Descendent is in  $19^{\circ} 29' 35'' 1''$ ; whence the Luminaries are between the Nonagesima and the descendent, or in the occidental quadrant; therefore the Visible Eclipse will happen after the true.

At one hour after Ecliptic $\odot$ App.	}	5 <sup>h</sup>	0'	14''
time at Sardis				
Moon in the Ecliptic	$\times$	9°	9'	52''
Her Lat. N. A.	0	4	21	
Sun's Long. then	$\times$	8	42	52
Parallax in Long. $\text{D} \hat{a} \odot$	0	43	28	
Visible ecliptic place of the Moon	$\times$	8	26	24
Parallax in Lat. $\text{D} \hat{a} \odot$	0	22	6	
Moon's Visible Lat.	0	17	45	
Visible Hourly Motion $\text{D} \hat{a} \odot$	0	20	22	
Interval from true to Visible $\odot$	1	47	38	
Apparent time of Visible $\odot$ at Sardis	5	47	52	
Mean time of Ditto at Greenwich	4	5	58	
Moon's Long.	$\times$	9	33	19
Her Lat. then N. A.	0	6	22	
Sun's Long.	$\times$	8	46	49
Parallax in Long. $\text{D} \hat{a} \odot$	0	52	35	
				Moon's



Moon's Visible Long.	✕	8°	40'	44"
Parallax in Lat. ☽ á ☉	○	0	11	49
Moon's Visible Lat.	○	0	5	27

At one hour before Visible ☿ at Sardis	4 <sup>h</sup>	47'	52"
Moon's Long. then	✕ 9°	3'	51"
Her Lat. N. A.	○	3	46
Sun's Long. then	✕ 8	44	22
Parallax in Long. ☽ á ☉	○	50	4
Moon's Visible Long.	✕ 8	13	47
Ditto at Visible ☿	✕ 8	40	44
Visible Hourly Motion of the Moon	○	26	57
Ditto of the Sun	○	2	27
Ditto of the Moon from the Sun	○	24	30
Parallax in Lat. ☽ á ☉	○	17	58
Moon's Visible Lat	○	14	12

At one hour after Visible ☿ at Sardis	6 <sup>h</sup>	47'	52"
Moon's Long. then	✕ 10°	2'	54"
Moon's Lat. N. A.	○	9	6
Sun's Long.	✕ 8	49	16
Parallax in Long. ☽ á ☉	○	50	43
☽ Visible Long.	✕ 9	12	11
Ditto at Visible ☿	✕ 8	40	44
Visible hourly motion of the Moon	○	31	27
Hourly Motion of the Sun	○	2	27
Visible hourly Motion of the ☽ á ☉	○	29	00
Parallax in Lat. of the Moon	○	13	48
Moon's visible Lat.	○	4	42

Angle of vis. way of the ☽ at visible ☽	19°	27'	38"
Motion from vis. ☽ to greatest obscuration	}	0	1 10
Vis. ☽ app. time at Sardis		5 <sup>h</sup>	47' 52"
Time from vis. ☽ to greatest ob- scuration	}	0	16 2
Vis. distance centers ☉ and ☽ at greatest obscuration		0	5 9
Vis. Motion of Semi-duration		0	30 17
			Angle

# TO VOL. THREE. 93

Angle of vis. way D from begin- ning Eclipse to greatest obscuration	19 <sup>h</sup> 38 6"
Vis. Motion in Long. from begin- ning of Eclipse to greatest obscuration	0 28 42
Time of Incidence	1 10 17
Time of Repletion	0 59 23
Semi-diameter of the Penumbra	0 31 11
Parts deficient	0 26 2
Digits eclipsed	9° 33' 57"

Whence the apparent Time of this Eclipse at Sardis on the 5th of March before Christ 618, happened as under,

Beginning P. M.	4 <sup>h</sup> 21' 33"
Greatest Obscuration	5 31 50
Visible &	5 47 52
End of the Eclipse	6 31 13
Duration	2 9 40
Digits obscured	9 33 57
Apparent time of Sun-set at Sardis, } was then	5 58 40
Greatest Obscuration before Sun-set	0 26 50

The eclipse would be visible 7 degrees east of Sardis, and the Sun would set there near 11 digits eclipsed.

## CORRECTIONS AND ADDITIONS.

Page 13. line 24. *For*, sent us; *read*, sent as.

P. 18. At end of first note *subjoyn*, and Diff. 14. p. 461. Tom. II.

P. 19. l. 11. *after*, Philip's letter *add*, in case Lous ever anticipated the equinox enough to commence so early as June 17.

Ibid. *Add in a note*, I make use here of the 3d Macedonian date, because it fell so much earlier in the year, than the two former Macedonian dates, not less than

## CORRECTIONS AND ADDITIONS.

than 18 days earlier: and hence a clear demonstration may be deduced, that the Macedonians must have inserted their intercalary month somewhere between the vernal and autumnal equinox, and not at the winter solstice as Dodwell supposes.

P. 23. l. 6. *For* to each other *read* one to the other.

P. 29. l. 11. *For* too late *read* too soon.

P. 62. in Note, l. antepenult. In some copies, on account of the ink not taking effect, will be found—48' instead of 48".

P. 63. *Subjoin in a note.* See Cicero de divinat. lib. I. and Proclus mentions another Chaldaean period of 270000 years. Ασσυριοι φησιν Ιαμβλιχος, εκ ικτα και εικοσι μυριαδας ιτων μονας ειλησαν, ως φησιν Ιππαρχος, αλλα και ολας αποκαλαστας και περιδους των ιπτα κοσμοκαλιων μνημη παριδουσαν. In Platon. Timæum Comment. lib. I. p. 31. We may learn hence, that the Chaldeans had a variety of similar vast periods for the motions of all the heavenly bodies. Accordingly another of 12054 years is mentioned by Cic. in Hortensio. By Solin. c. 33. Servius in æn. 3. 284. Tacitus Mor. Germ. Cicero mentions another of 3000 years *de nat. dev.* M. Bailly shews in his *Astronomie Orientalee* that the last is still found subsisting in Indian Astronomy, as also are the Chaldean periods of 600 years and 36000, mentioned by Josephus and Syncellus: Possibly others may be discovered hereafter. Astronomic and astrologic Periods were the Fashion in Chaldaean Times.

P. 64. *Add in a note.* M. Boulanger seems to have been of the same opinion, that the Olympic games were originally celebrated on some astronomic account relative to the termination of a solar or lunar period. "On peut mettre au nombre des solemnités Jubiilaires quelque-unes des plus celebres fêtes des Grecs et des Romaines, qui se celebrant apres quatre années revolues, doivent être regardées comme des Jubiles lunaires de sept fois sept ou de 49 mois; tels étoient les jeux Olympiques en Grèce, et les Jeux capitolins à Rome." *L'Antiquité dévoilée par ses usages. Tom. III. p. 33.*

P. 70. l. 13. *after* Calendar: *add*; much less when thus reckoned backward 850 years before its first institution.

P. 92. l. 10. an erasure in the copy makes it doubtful, whether the figures 40, should be 46 or not.



# ILLUSTRATIONS

OF THE

A P P E N D I X

TO VOLUME THREE

OF CRITICAL OBSERVATIONS ON

B O O K S

ANTIENT AND MODERN.

CONTAINING

Chiefly, Proofs from Ptolemy, that the calippic Years began at the autumnal Equinox; whence it necessarily follows, that each of the Six autumnal Equinoxes observed by Hipparchus and dated by calippic Years, must have been made One Julian Year later than M. de la Lande and others have supposed: consequently those Equinoxes must have fallen Six Hours later than M. de la Lande has computed. Hereby they become vindicated from the chief Part of the Inaccuracy so harshly imputed to them by the Moderns, which is thus proved to have arisen from a Mistake in modern Chronology, not in antient Astronomy.

Avia doctrinæ peragro loca, nullius antè  
Trita solo; juvat integros accedere fontes.  
Nec me animi fallit, Graiorum obscura reperta  
Difficile illustrare antiquis testibus esse;  
Sed sciendæ ardor quemvis perferre laborem  
Suadet, & inducit noctes vigilare serenas;  
Quærentem dictis quibus, & quo carmine demum  
Clara tuæ possim præpandere lumina menti,  
Res quibus occultas penitus consilere possis.  
Hanc igitur noctemque animi tenebrasque necesse est,  
Non Radium solis, neque lucida tela Diei  
Discutiant, Rerum vero species Ratioque;  
Quas cum pernosces parvâ perfunctus opellâ  
(Namque aliud ex alio clarescet) non tibi cœca  
Nox iter eripiet, quin ultima veritatis  
Pervideas, ita Res accendent lumina Reribus.

LUCRET. 4 & 1.

L O N D O N:

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CLIMATE

AND

ENVIRONMENTAL SCIENCE

AND

PLANETARY SCIENCE

AND

SPACE SCIENCE

AND

ATMOSPHERIC SCIENCE

*Illustrations concerning various Subjects treated of  
in the Appendix to the Third Volume.*

**I**N such periodical publications as these of my *Critical Observations*, the first part of each number of which is generally printed off before the last part is committed to paper, it can not be reasonably expected, that a writer should be able at the moment of composition to recollect *every* information, evidence or argument, which may have formerly occurred to him concerning the several subjects treated of therein; nor to support with *proofs* all the *assertions*, which he may have any where made there, and assumed as foundations of his reasoning, from whence various conclusions are deduced: nor even if I could recollect every thing precisely at the moment when wanted, could I either find room for all, or be able to introduce such a variety of matter without perplexing and interrupting the chief argument and object in view. Moreover, I have long ago disclaimed all intention of beginning to treat of any subject from the very first foundation of it, that is, from the very *institutions* of each respective science; and I only proposed to take up subjects just at that point, where others had begun to make mistakes in them, and to perplex them with adventitious errors, no way necessarily inherent in the subjects themselves; in order that I might shew how they may be extricated out of those errors, and instead of the confusion wherein they have hitherto been involved by others, how they may be advanced



to higher degrees of perspicuity, harmony and certainty. Those errors, however, I have found to be so very numerous, and they do often so totally alter the whole state of various subjects which come under consideration, that I can not find time to enter into an examination and refutation of all even of them; I proposed, nevertheless, to treat occasionally, in separate *Dissertations*, of some of the most important of such errors and of other subordinate subjects thus omitted before, whenever any thing new or more certain and perspicuous can be said concerning them\*: Accordingly the present *Illustrations* are intended to supply such information as may still be wanting concerning some subjects in the *Appendix* to Vol. 3, and to explain and prove in others the truth and accuracy of various assertions to be found there, in which I may differ from writers of character; the proofs of which could not, without interruption, be introduced before, but concerning which it may now be acceptable to readers to receive some farther satisfaction, reference being made to the pages of *the Appendix*, where the respective passages occur, which are to be thus further enlarged upon.

P. 5. "*Petavius has totally rejected the idea of any such fact.*" - His words are, "Fictum illud est a Scaligero de traducto anni calippici capite à Solstitio in autumnum [a Macedonibus] quod nunquam Calippus instituit; cujus periodus à Solstitio incipiebat, non ab autumnò: sed nec alius quispiam Calippicam illam periodum im-

\* See vol. ii. p. 114, and elsewhere.

mutavit aut eam inchoavit aliundè—dixi non semel et adhuc dicendum est calippicam periodum non alio anno quam eo, qui ante mortem Alexandri septimus est, periodi Julianæ 4384 [ante chr. 330] ab solstitiali mense lunari initium duxisse." *Doctr. temp. lib. 2. 22* \*. Nevertheless, Dodwell afterwards revived Scaliger's opinion, that the calippic period, as adopted in Asia by the Macedonians, was different from that which obtained at Athens; for that in Asia its year began at the autumnal equinox, and at Athens at the summer solstice: yet Dodwell seems to have collected no better evidence of this pretended difference than Scaliger had before †. But Scaliger supposed moreover, that in Asia the calippic period com-

\* Observe, that Petavius rightly here calls the year 330 the *seventh* before and exclusive of that in which Alexander died; his death was in June, in the 323d year before Christ. I thought it proper to notice this, because in his *chronologic tables*, at the end of his *Doctr. temp.* he has erroneously placed the death of Alexander one year *too soon*, viz. in the 324th before Christ; this error he attempts moreover to defend, in a *Dissertation* among his *Paralipomena* at the end of his second volume, under the title *de ortu et obitu Alexandri*; but it is indefensible. This is the cause of the difference of a year between Petavius's date of Alexander's death, and mine in my table at p. 353 of vol. i. Petavius himself seems afterwards to have suspected his error, for in his *Rationarium temp.* composed many years later, he says, that Alexander died *ant. Chr. 324 vel saltem 323*. See *pars secunda lib. i. c. 15*.

† "Coepit cyclus Macedonum calippicus anno olymp. 112. 3 Sept. 26;" i. e. in the year 330 before Christ. *De vet. cycl. diff. 9. sect. 16. p. 391*.

menced in the year 331 before Christ, that is from the victory at Arbela in that year, about the beginning of October; yet at Athens only from the summer solstice in the year 330: in this imaginary difference Dodwell, however, did not follow Scaliger. Now in regard to these discordant opinions, I find, that most of the dates by the calippic period in Ptolemy prove, that it commenced in 330; and that one alone (where there is however great room for suspecting the dates to be corrupted) tends to prove, that it commenced in 331: as to the *season* also of commencement, there is almost demonstrative evidence in Ptolemy in favour of the autumnal equinox, and *no certain* evidence to the contrary, nor yet the least indication of any difference between the methods adopted at Athens and in Asia; if indeed the calippic period was ever in popular use at either place, which, as I conceive it never was, but was only employed as an accurate scientific calendar, soon after its invention, by Timocharis and a few other astronomers in Egypt, some of whose dates by it are quoted by Ptolemy\*.

P. 5. " *They*

\* Here we may observe how strangely opposite has been the fate of the æra of Nabonassar and that of the calippic period: for, concerning the former the learned have determined, that it was only a scientific æra invented by astronomers in Egypt for their own use, and never employed as a vulgar popular æra any where; notwithstanding there are so many evidences still remaining, as I have shewn, that it was the vulgar æra and popular mode of computing time, not only in Egypt, but throughout Asia, Persia, and Chaldea; and



P. 5. " *They have all contended, that some passages in Ptolemy prove the calippic period to have commenced*

and that the chief constituent parts of the method of computation employed in the æra of Nabonassar subsisted in vulgar use, both in Egypt and in Persia under the last race of kings there, until their conquest by the Arabs so late as the year 632 of Christ, and even later. On the contrary, in regard to the calippic period, they have contended, that it was in vulgar use not only at Athens and in Macedonia, but in Egypt and throughout Asia as far as Babylon; and yet there is not the least proof to be found of this fact in ancient authors, no not even with respect to Athens. For if the quotation by Ptolemy, of some astronomic observations in Egypt, by Timocharis and Hipparchus, dated by the calippic calendar, can prove its popular use there or elsewhere; then the popular use in Egypt of the calendar of Dionysius Mathematicus, by celestial months, may be equally proved from Ptolemy: for he often quotes astronomic observations, dated by the calendar *κατα Διονυσιον*, which began to be employed about 283 years before Christ; the chief circumstance of which was, that the number of days in each month was made equal to the number of days which the sun took in passing through each sign of the zodiac. But there is no evidence, that this calendar was employed by any except astronomers and scientific men, just as is the case with Scaliger's Julian period: Hipparchus made use equally of the calendars by Dionysius and Calippus, *c. gr.* " *Πάλιν ετους καὶ κατα Διονυσιον Λεόντωνος καὶ ἑσπερας προηγείτο του σλαχους, ἐξ ὧν ὁ Ἰππαρχος ἐπιλογιζετο, &c. Anno rursus 24 secundum Dionysium Leontonis mensis die 28 vesperi præcedebat Spicam [Mercurius] ut Hipparchus computavit, &c. (lib. ix. c. 7).*" How then can it be concluded from Ptolemy, that the one calendar was in vulgar use more than the other? No dates are to be found any where by the calippic calendar, except these  
in

*commenced at the summer solstice, which in fact none of them do.”]* Scaliger, Petavius, and others all refer to the subjoined passage in Ptolemy, as containing proof in favour of the summer solstice; although in reality it proves nothing more for the summer solstice than for the autumnal equinox, and only that the calippic period must have commenced *later* than the summer solstice, but not so late as the winter solstice: for this passage is just as compatible with the opinion of those who fix the commencement of the calippic year at the first new moon *after* the autumnal equinox, as of those who fix it at the summer solstice\*. Now Ptolemy here says, “that Hip-

in Ptolemy by astronomers: and Diodorus expressly says, that the calendar of Meto continued *generally* in use down to his own time among the Greeks, “*Græcorum plerique Metonis Enneadecateridos rationem amplexi ad hæc usque tempora à veritate non aberrant,*” *lib. 12. apud olymp. 86. 4.* Now what do writers reply to this passage? only that though Diodorus says Meto, yet that he meant Calippus: at this rate we may prove, that men walk on their heads, and not on their feet. What other objections and evasions they make to this evidence of Diodorus, I may find some other opportunity to consider, but they are just as futile as in the above desperate supposition.

\* Συγκρίνας [Ἰππάρχος] τὴν ὑπὸ Ἀριστάρχου τετηρημένην θερὴν τροπὴν τῷ ν εἰσι ληγόντι τῆς πρώτης κατὰ καλῖππον περιόδου, τῇ ὑφ’ ἑαυτοῦ παλιν ἀκριβῶς εἰλημμένη τῷ μγ εἰσι ληγόντι τῆς τρίτης κατὰ καλῖππον περιόδου, φησὶν οὕτως, &c. *Observatum ab Aristarcho solstitium 50 anno primæ secundum Calippum periodi exeunte conferens Hipparchus illi solstitio, quod ipse 43 anno 3tiæ secundum Calippum periodi exeunte cepit, sic ait, &c. lib. 3. sect. 2. p. 63. Edit. Basil. 1538.*

parchus

parchus had compared the summer solstice, as observed by Aristarchus in the 50th year *declining* of the first calippic period, with the same solstice accurately observed by himself in the 43d year *declining* of the 3d calippic period." Hence all those authors above mentioned erroneously conclude, that since the calippic years in question, in both these observations, are mentioned as being *declining* when the observations were made at the time of the summer solstice, those calippic years must have consequently *ended* with the Greek months then current, which must have been the month Scirrophorion in both cases; and which ended in the year of the *first* observation 18 days after the summer solstice: as may be discovered by my calippic tables at No. 50, where it appears that Scirrophorion began in that year at the new moon on June 16 and ended on July 15; so that the solstice on June 27 fell a little before the *end* of that month then current. In the case of the *second* observation likewise in the 43d calippic year, Scirrophorion began at the new moon on June 3 (or 2) and ended on July 3, about 5 or 6 days *after* the solstice on June 27. The above authors therefore would confine the word *ληγοντι* to the sense of *just upon the point of ending*: thus Scaliger expressly understands and translates it, i. e. *jam desinentem* \*. Christmannus also says, "Calippus periodum suam a solstitio exorsus est, ut ex Ptoletheo colligere licet; is enim scribit observationem sol-

\* Lib. 2. de periodo calippica attica solstitiali. p. 87. Edit. 1629.



stitii ab Aristarcho factam, incurrisse in annum 50 periodi Calippicæ jam desinentem." (*In Alfragan: commentar: p. 305, ann. Chr. 1590*). Petavius reasons likewise in the same manner, "Ptolemæus scribit observatum ab Aristarcho solstitium τῷ ὧ ἐτεὶ ληγόντι; ergo circa solstitium desinit annus calippicus." *lib. 2. 19. Edit. 1627. p. 136.* Again in *lib. 10. c. 36.* "Labente itaque Scirrophorione ejusque circiter duodecimo die, Aristarchus solstitium deprehendit: ideo dixit Ptolemæus desinente anno 50." But here they all attach to ληγόντι the sense of *almost ending*; of which it is indeed capable; but it is not the *necessary* sense of the word; for it was also employed by the Greeks in a larger signification, to express any time whatever of a period, if it was but *declining*, that is, if a full half of the period was expired. Thus Pollux, distributing a year into three parts, employs ληγόντος to express the *whole third* and last part of it, "Τὰ δὲ μέρη τοῦ ἐτους, ἴσταιμενου, μεσούντος ἢ ληγόντος \*." Again soon after, distributing the Greek months into three parts, agreeably to the common practice, he employs ληγόντος as synonymous to φθινόντος to express the *whole third* and last part of a month, "Μέρη δὲ μηνός, ἴσταιμενου, μεσούντος καὶ ληγόντος," (*Secl. 5.*). Nay Dodwell himself, although he agrees with the others above-mentioned in regard to Ptolemy's meaning of ληγόντος, yet he employs ληγόντος in the very same extensive sense as Pollux, "Cum dictum est Δευτέρα ἴσταιμενου, μεσούντος, ληγόντος, vox prior δευτέρα ad μηνὴν (lunam fem.) al-

\* Onomasticon, lib. 1. c. 7. Sect. 3.

tera ἰσόμενου ad μηνος (*mensē* mascul.) refer-  
tur;" *Diff.* 2. p. 106: And nevertheless, he  
adopts the erroneous conclusion of Scaliger  
and Petavius as deduced from the above words  
in Ptolemy. But if ληγοντος is thus able to de-  
note a full *third* part of a *declining* year, why is  
it not equally able to mean in Ptolemy the still  
less sum of a full *quarter* of a year? Certainly  
for no other reason than an imaginary prepos-  
session in favour of the summer solstice; for  
Ptolemy's expression by ληγοντι may just as well  
signify, that the calippic year ended at a *full*  
*quarter* after the solstice, namely, at the au-  
tumnal equinox, as at the end of the Greek  
month Scirrophorion then current, and a *few*  
*days only* after the solstice: so that all the con-  
clusion which can be deduced from this passage  
in Ptolemy is, that more than a *half* of a ca-  
lippic year was expired at the summer solstice,  
and that it must have ended *later* than that sol-  
stice; but whether only a *few days* later, or 1 or  
2 or 3 *months* or more later does not appear, and  
only that it must have ended not *so late* as the  
winter solstice, this being a *full half year* after  
the summer solstice.

I have been more particular in quoting the  
very words of the above authors, lest readers  
should suppose it impossible, that so many  
learned men could have placed so much depen-  
dence upon so very weak an evidence; and that  
from Scaliger down to De la Lande, they should  
have deduced such important consequences as  
they have done from such unsolid premises. Yet  
thus inconclusively and inconsistently do men of  
learning often reason, and thus in their argu-  
ments

ments do they often contradict even their own words and opinions elsewhere : And on no better foundation than such unsolid evidence as this, have they erected tottering superstructures in astronomy as well as in chronology, in order to support which they alter, add to, erase, interpolate the dates and words of Ptolemy and other ancients at their pleasure ; sometimes, like Dr. Priestley, forcing them to say the directly contrary to what their words obviously imply ; at other times they clearly see one half of the sense of an antient quotation, but yet like Dr. Priestley again, they become quite blind to the other half, in case it makes against them. What then is all this but learned vision and romance ? the mere effect of a spirit of jealousy, prejudice and disputation, not of an ingenuous zeal to discover truth in whatever authors it can be found ! To this, neither Unitarians, nor any other such public disputers in other parts of knowledge can form any real claim, notwithstanding all their pretences. Thus do we spend one half of our lives in studying and appealing to the authority of other learned moderns ; and then it costs us the remaining half to study the original antients themselves, in order to discover and unlearn those errors again, into which such high German artists in literary juggling and conjectural reformations have before misled us ; so that we generally arrive at the end of life, before we have been able to arrive half way towards the goal of truth ! For this reason I shall continue, while health and leisure continue, to contribute my own share of attention to facilitate the studies of others, by endeavouring in  
many



many fundamental articles to sift and separate real truth from error : and I have repeatedly experienced, that truths wheresoever discovered, in whatever authors, or in whatever sciences, even although in such as at first sight seem totally unconnected with each other, yet somehow or other they nevertheless will be found to have a connexion with or a tendency to illustrate and confirm each other ; for, indeed harmony is of the very essence of truth, while on the contrary, the necessary effect of error is discord. In fact, it is with truths as with musical sounds, for if you touch one string, it will cause some other which is in unison with it, to vibrate at the same time. Accordingly, I have had already frequent occasion to shew, by particular examples, that whether we have recourse to Herodotus, or to Ptolemy, or to the Parian Marble, or to my calippic tables, or to other antient monuments or authors, in order to correct current errors ; whether we make researches concerning prophane customs or Christian antiquity, or concerning history, or chronology or astronomy, or heretical Christian tenets, or the meaning merely of Greek words and phrases ; yet they do all in their several turns contribute to my chief proposed object in view : now this was, to illustrate and confirm the originality, authenticity, consistency and veracity of the divine scriptures of the Jews and Christians, and also of prophane historians still extant ; with all of which antient evidences the quackery of learned conjecture daily attempts as many rash and licentious innovations as we shall find that it does with the Greek text of Ptolemy.

Ptolemy. The necessary consequence of this conduct must be, that at last we shall probably lose the original text of all prophane authors; just as the Jews and Christians in the very first centuries had lost the original text of the Septuagint; and this through the same means, that is, the licence which every Jewish and Christian reader took in the first two centuries (before the time of Origen) to correct his own manuscripts of the Septuagint; either in conformity to his own ideas of the supposed meaning of the Hebrew text, or his own critical conjectures of what the sense *ought* to be, or else in conformity to some antient translation of the Septuagint: the original text of the Septuagint may, indeed, be still subsisting in some of the different Greek manuscripts now extant; but then we know not how to distinguish it from the innovations and corruptions of such pretended reformers. The same must soon be the case with prophane authors of antiquity, if such desperate conjectures both concerning the *text* and its *meaning*, be received with approbation, as have been attempted in regard to Ptolemy by Scaliger, Christmannus, Petavius, and Dodwell.

From what has been said, however, in the preceding paragraphs, I do not mean hastily to conclude, that the calippic period *certainly* began at the autumnal equinox, but only that the above passage in Ptolemy, to which *alone* in reality all chronologers have referred (notwithstanding they have said, that there are *many others* to the same purport) contains no proof of its commencement at the summer solstice; that passage being equally compatible with *either* of the two opinions: and whatever is really

really still doubtful, ought to have been represented by those authors no otherwise than as doubtful; especially since none of them have produced *any other* evidence in favour of the summer solstice than what results from the above quoted passage in Ptolemy; neither indeed have I myself met with any other evidence in Ptolemy or elsewhere. Hence then arises a *presumption* at least in favour of the autumnal equinox, and this presumption we shall find to be confirmed by two very strong *proofs* in Ptolemy, though indeed not indubitable ones: one of them however is of such a nature, that I know not how to resist the force of it; this proof therefore I shall here subjoin, for it requires our strict attention\*. Scaliger had long ago

\* Μεταβησομεθα δὴ καὶ ἐπὶ τὰς ὑστερον ἐκτεθειμενας αὐτῷ [Ἰππαρχῳ] τρεῖς ἐκλείψεις, ὥς φησὶν ἐν Ἀλεξανδρείᾳ τετηρησθαι. Τούτων τὴν Πρωτὴν φησὶ γεγενεσθαι τῷ ΝΒ εἰς τῆς δευτέρας κατὰ καλλιππὸν περιόδου, κατ' Αἰγυπτίους Μεσσηρ 15—[Here intervene 9 lines in Ptolemy's text]—Τὴν δὲ ἐξῆς ἐκλείψην φησὶ γεγενεσθαι τῷ ΝΕ εἰς τῆς αὐτῆς περιόδου, κατὰ Αἰγυπτίους Μεχέρ 5—[Here intervene 11 lines in Ptolemy's text]—Τὴν δὲ Τρίτην φησὶ ἐκλείψιν γεγενεσθαι τῷ ΑΥΤΩ ΝΕ εἰς τῆς δευτέρας περιόδου, κατὰ Αἰγυπτίους Μεσσηρ Ε. Sed transeamus ad tres eclipses postremo ab illo positas, et Alexandriæ observatas ut ait; quarum primam asserit fuisse anno 52 secundæ secundum calippum periodi Mesore secundum Ægyptios die 16—Sequentem autem eclipsim 55 anno ejusdem periodi factam asserit Mechir secundum Ægyptios die 9—Tertiam eclipsim fuisse ait eodem anno 55 secundæ periodi Mesore die 5, secundum Ægyptios, &c. lib. 4. scilicet. ultim.

adverted



adverted to the passage in Ptolemy, which contains this proof; and he attempted to deduce from it the two following facts: *First*, that the calippic period began in the year before Christ 331, among the Greeks in Asia, though not at Athens. *Secondly*, That in Asia it began at the autumnal equinox. For as the eclipses mentioned in this passage were observed among the Macedonians at Alexandria, he supposed them to be dated agreeably to the Asiatic mode of the period of Calippus, and that this was *different* from the calippic period at Athens: but if there was no difference in the calippic period at Athens and Alexandria, then the dates of these eclipses become proofs concerning the *year and season* at which the calippic period commenced at Athens and every where else, wherever it was employed as well as in Egypt.

As to the *first* fact, that the calippic period commenced among the Macedonians in 331 before Christ, not in 330 as at Athens; in this, as I apprehend, Scaliger was certainly mistaken; the dates here referred to by Scaliger, would indeed prove the fact he contends for, if they could be depended on as not being corrupted; but Scaliger himself allowed that the *first* of these three dates, namely, that of 52 ( $\nu\beta$ ) was a corruption instead of 54 ( $\nu\delta$ ): And Petavius, when he came afterwards to refute this proof of Scaliger for the commencement of the calippic period in 331, contended, that the *second* date 55 ( $\nu\varepsilon$ ) was a corruption likewise by the copiests; for that it should be 54 ( $\nu\delta$ ) like the former date. It seems to me, moreover, very doubtful, whether all the *three* dates have not been  
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corrupted; so that nothing *certain* can be deduced from any one of them, either in favour of the year 331 or 330\*. But at least the evidence arising from these dates in favour of 331, even if it were not thus doubtful, is contradicted by *all other dates* in Ptolemy; which depose clearly and harmoniously in favour of

\* “Mendosos esse Ptolomæi numeros quovis pignore contenderim. Nam prioris eclipsis annus in Græco codice notatur 52 (vβ) pro quo Latinus interpretes 54 posuit, et rectè quidem—In secunda pro 55 (vi) rescribo 54 (vδ) ut idem annus [eclipsis prioris] continuetur usque ad secundam eclipsim in martio mense et die 19.” *Lib. 2. c. 18. p. 135.* The 3d date he supposes to be right, viz. 55: but this I doubt; for if the 55th calippic year of the 2d period ended at the autumnal equinox in the Julian year 200 before Christ, then the first calippic year of the first period must have begun at the autumnal equinox in the year 331 before Christ, which is contradicted by *all* the other calippic dates in Ptolemy; accordingly it was for this very reason produced by Scaliger as a proof, that the calippic period among the Macedonians *did* commence in the year 331. Now if *two* of these dates be allowed to be corrupted, I see no dependence which can be placed on the *third*; so that they seem *all three* to have been corrupted, and possibly they were originally 53, 54, 54, instead of 52, 55, 55: but at present they can afford no evidence *either* way, except by means of lawless conjecture. This, however, must be observed, that if the calippic year began at the solstice, then the *third* date 55 would be right, and the calippic period would commence no earlier than in 330: for this reason Petavius attempted no rectification of the *third* date; but in this case the 2d and 3d eclipses would not fall in the *same* calippic year, as we shall see they are declared in Ptolemy to have done; so that there is nothing but discord, which ever opinion be adopted.

330. However, as this subject concerning the year, when the calippic period commenced, is foreign to the present matter in hand, I shall consider it no farther; but proceed to the *second* fact, which Scaliger attempted to deduce from this passage, and which rests upon a more secure foundation than doubtful numeric letters, namely, the *season* of year, at which the calippic period commenced in Asia at least, and that it was at the autumnal equinox, not the summer solstice.

The proof of this *second* fact Scaliger deduces from the following circumstance, that the *second* and *third* lunar eclipses in this passage are here marked by the *dates* as if *both* of them happened in *the same* calippic year 55 (*ve*). Now Petavius and all astronomers allow, that the *first* eclipse happened on Sept. 22, in the year 201; that the *second* eclipse could happen at no other time than about midnight, between March 19 and 20, in the Julian year before Christ 200; and that the *third* eclipse must have happened in the same Julian year 200, on Sept. 12: if then *the same* calippic year 55 was still current from March 19 until the following Sept. 12, it evidently follows, that no new calippic year could have commenced at the summer solstice, nor at any other point of time between March 19 and Sept. 12. But then in order to set aside this conclusion, Petavius contends, as we have seen, that as the first date has been possibly corrupted from 54 to 52, so also the second date may have been corrupted from 54 into 55; which, if true, would doubtless set aside the above whole proof of Scaliger.

Now,



Now, so far this reasoning is indeed true, that there is great reason to suspect, that the *second* date may not be correct; because the *third* date is apparently no more correct than the 1st or 2d, for, as above-mentioned, instead of 52, 55, 55, the dates probably ought to be 53, 54, 54; but then all this is matter of great uncertainty, therefore no satisfactory evidence can be derived from the *dates alone* in this passage either one way or other, either in favour of Scaliger or of Petavius. Scaliger died in 1609, and thus never lived to see the pretended refutation of *this proof* by Petavius, whose *Doctrina temp.* was first published in 1627; hence no reply was ever made to the above reasoning of Petavius: and although it is indeed insufficient, yet, by not being refuted, the reputation of Petavius has caused his own opinion on this subject to be received with a more implicit assent by other moderns than it deserves; and it has been upon this insecure foundation thus laid by Petavius, that M. de la Lande has depended in his reasonings concerning the six autumnal equinoxes observed by Hipparchus \*. If Scaliger had

\* How universal has been the prepossession of the moderns concerning the commencement of the calippic period at the solstice, (not only without any evidence, but even against all evidence now extant, and for no other reason than because Meto's period commenced at the solstice) we may collect from the following passage in Selden's *Apparatus Chronologicus* to the Parian Marble, published soon after Petavius's *Doctr. temp.* in this Selden and his assistants in that *Apparatus*, speak so magnificently of Scaliger and Petavius, as not only shews

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had survived the publication of Petavius's *Doctrina temp.* he might, and probably would have easily refuted the objections of that author to *this proof* in Ptolemy of the calippic period commencing at the autumnal equinox \*. For Scaliger might have allowed, that he himself had acted incautiously in making the strength of *his own proof* depend chiefly upon the accuracy of the *dates* in the present text expressed by numeric letters; which, as it is well known, have been subject to more errors by copiests than any other letters; but he might have nevertheless safely maintained that *his proof* was still a

the implicit deference paid in that age to their sentiments; but also they speak so decidedly concerning the opinion itself in question, and the many pretended proofs of it existing in Ptolemy, that one would scarcely suppose the possibility of their having never consulted Ptolemy's own work, in which no such proofs are to be found. "Periodus calippica incepit sub æstivum solstitium, quod satis ex Ptolemæo non uno in loco liquet: id item pluribus et expressius, post summum virum Scaligerum ostendit pridem vir sanè etiam summus Petavius in secundo de Doctr. temp. libro cap. 18." *Marmor. Arundel. p. 66. (ann. 1629.)* vide etiam *Prideaux Marm. Oxoniens. p. 236.* But these great scholars had so many prepossessions in favour of particular opinions, and such prejudices against some others, that the clearest evidence was often overwhelmed by the strong power of their different imaginations and conjectures.

\* Scaliger expresses in the following manner the conclusion, which he draws from *this proof* in Ptolemy in favour of the autumnal equinox, "Videmus hic a Virgine ad Pisces mutationem anni factam, a Piscibus ad Virginem non factam: ergo annus calippicus [apud Macedones] cœpit ab autumno." *Lib. 2. de periodo calippica ab autumno. p. 91. edit. 1629.*

good

good one : for at the *third* date it is expressly said, in words at length, that the eclipse there mentioned, happened in *the same* calippic year as the *second* eclipse [*ἐν τῷ αὐτῷ ἔτει*]. Now let all the three *dates* have been originally whatever any writer shall please to conjecture, yet this cannot alter the evidence arising from the word *αὐτῷ*; this word will still in every case prove that the *second* and *third* eclipses were both in the very *same* calippic year; therefore the calippic year could not commence at any season between March 19 and September 12 : it has fortunately happened also, that Ptolemy has here so fully compleated his sentence as to prevent any ambiguity, by saying, *τῷ αὐτῷ ἔτει τῆς δευτέρας περιόδου*; for hereby all pretence is prevented, that he might mean by *τῷ αὐτῷ ἔτει* the *same year of Nabonassar*, as he had just before mentioned, and not the same calippic year.

I see no method then to avoid the conclusion arising from the word *αὐτῷ* in favour of the autumnal equinox, except by such unwarrantable conjectural alterations of Ptolemy's text, as would destroy the integrity of any author whatever; and enable a writer to prove from any quotation just whatever he pleases : accordingly to these lawless contrivances Petavius has been forced to have recourse here, in order to evade the force of Scaliger's conclusion from this proof in Ptolemy; which kind of contrivances Bossuet some where emphatically calls *the miserable subterfuges of disputation* \*. In my *Appendix* at p. 5, I have said, *that no writer had adverted*

\* Les misérables échappatoires des disputans.



to some of the proofs contained in Ptolemy in favour of the autumnal equinox; this I said, because Scaliger himself in none of his editions had made the least remark concerning the word *αὐτῶ*, although he had faithfully translated it: neither is the least notice taken of this word in that chapter of Petavius referred to above, where he attempts to refute *this proof* produced by Scaliger; hence I concluded, though indeed erroneously, that it had escaped the attention of both of them. But I have since perceived, that it is noticed afterwards by Petavius; yet in a place where I did not expect to find it, viz. in his second vol. in which he recurs again to the former subject *de initio periodi calippicæ*: and in order to evade the proof arising from the word *αὐτῶ*, he pretends there, that it is an error in the text; for that this word belongs to the account of the *second* eclipse, whence it has erroneously been transferred to the *third* by the copiests\*. If such conjectural transpositions as these are allowable in the text of an antient author; if a reasoner may thus remove every thing, which makes against him, and introduce any thing which makes in his favour, it is certainly very easy to prove any opinion whatever. But Petavius ought however to have farther informed his readers, that this vast jump, which he forces the word *αὐτῶ* to take backward, is

\* “ In *secunda* defectione Ptolemæus scripsit τῷ αὐτῶ ὅδ' ἐτεῖ τῆς αὐτῆς περιόδου. In *tertia* non dubitem scripsisse Ptolemæum aut Hipparchum τῷ ἢ ἐτεῖ τῆς δευτέρας περιόδου, non ut vulgo τῷ αὐτῶ ἢ ἐτεῖ.” lib. 10. c. 37.

over no less than 11 full Greek lines, in a small type, of a full folio page; see the whole quotation at p. 107. Thus we find, that this author having taken up an ill-founded supposition, that the calippic period commenced at the summer solstice, upon the weak foundation of the word *ληγοντι*; he afterwards does not scruple to heap mountains upon mountains to defend his false opinion, and even makes a desperate attempt to expel that inimical word *αυτω* out of the sentence in Ptolemy, where it occurs: so that he *ends* with being a conjectural conjuror in the art of correcting antient authors, who only *began* with being an inattentive critic concerning the extensive sense, in which *ληγοντι* was employed by the antients; and such is too often the case with all other literary disputants. How then can public disputation ever lead to truth? Petavius probably had not thought of this admirable contrivance to get rid of a word, which stood directly in his way, when he wrote his chapter on this subject in his *first* volume: neither indeed is it his own invention, but borrowed from Christmannus; whom he often quotes by name on other occasions, although he has here suppressed the prior claim of Christmannus to this extraordinary device\*. Here however one cannot help expressing one's admiration at perceiving the great and distant consequences,

\* In his notes on Alfraganus under the title *de connexione annorum*, p. 327. published ann. Chr. 1590.

which do sometimes follow from little causes; for if the supposed flight of this little word *αὐτῷ* should not be approved by the public, then the consequence will be, that M. de la Lande must have apparently placed all or most of the six autumnal equinoxes observed by Hipparchus a year *sooner*, than the observations were really made. For the time of observing those equinoxes is dated *only* by the years of the calippic period; but if the word *αὐτῷ* be now in its *right* place in Ptolemy, the calippic years must have begun at the new moons next *after* the autumnal equinoxes: consequently every one of these six dates by the calippic year would place those equinoxes a whole Julian year *later* than M. de la Lande supposes, namely, at the *end* of each calippic year there mentioned, not near the *beginning* of each; and this variation will derange the whole of his pretended accuracy of calculation concerning the length of the solar year, as deduced from those six observations of the autumnal equinoxes by Hipparchus\*. Thus do we find again, that even in astronomy, as well as in other sciences, authors blindly follow the authority of their predecessors even in their extravagancies and romances! and none of them seem to think it requisite to restrain their learned cogitations to the mere *caput mortuum* of truth; nor to call in to their assistance the sober corrections of chronology in order to discover truth! On the contrary, all classes of writers are equally ready to swim along the

\* Memoire sur la veritable longueur de l'année astronomique, Copenhague, 1784. p. 14.



current stream of error, and are all equally disposed to say with Lucretius to a rare felicity of ingenious *Invention*, "Te dea sequimur, nam tu sola potes juvare mortales." On what a slippery foundation then does our knowledge in prophane antiquities rest at present? In reality, the very ground shakes under us every step as we advance; the farther we proceed, the more we become entangled in a wilderness; and we often seem to tread in the imaginary scenes of Fairy land, instead of resting upon the solid rocks of certainty and facts.

But even if this slight puff of wild conjecture from the mouth of Petavius concerning the transposition of *αυτω* should meet with a favourable reception from the public,\* yet there is another *date* in Ptolemy, which again equally testifies in favour of the calippic period commencing at the autumnal equinox: Scaliger indeed has taken no notice of it, but it did not escape the observation of Petavius; and he has been forced here a third time to have recourse

\* Scaliger's 2d edition of his *Emend. temp.* was published in 1593, his 3d and the last, during his life-time, in 1598: he might therefore have noticed and censured the conjecture of Christmannus concerning *αυτω* being transferred from its right place. Scaliger died in 1609; he could not then refute the approbation of that conjecture by Petavius, whose *Doctr. temp.* was not published until 1627. The edition of Scaliger in 1629 was a posthumous one, made from a copy corrected in the hand writing of Scaliger. Hence the errors of Petavius have remained ever since unanswered and uncensured; and have become the foundation, on which De la Lande has made his calculations.

to his former practice of altering the text of Ptolemy in order to evade a proof which made clearly against him \*. Here Ptolemy informs us, "that an observation of the moon by Hipparchus on the 16th of the Egyptian month Epiphi, in the 50th year of the 3d calippic period, fell on the 315th day of the 620th year of the æra of Nabonassar." Now this 620th year began on Sept. 24, in the 129th year before Christ, therefore the *end* of the 315th day after and inclusively coincided with Aug. 5, in 128; at which time the 50th year of the 3d calippic period was current. If then to these 50 we add the 152 years of the first two calippic periods, they make 202 years in all; and if that 50th year continued current until the autumnal equinox after August 5, we must add those 202 years to the  $127\frac{1}{4}$  years before Christ (reckoned back to the autumnal equinox in 128) which make in all  $329\frac{1}{4}$ ; that is, it hence follows, that the *first* calippic year could have begun no earlier than at the autumnal equinox in the 330th year before Christ, a  $\frac{1}{4}$  of a year before the 329th before Christ; which is right and agreeable to other proofs. Such must be the conclusion, in case the 50th year current on August 5 *ended* at the

\* Ἡν [σεληνὴν] φησι [Ἱππάρχος] τετηρημένην τῷ ν ἐπέ-  
της τρίτης κατὰ καλῖππον περιόδου, κατ' Αἰγυπτίους Ἐπιφ-  
15—συναγεται τοίνυν καὶ ἐνταῦθα ὁ ἀπὸ τῶν ἐποχῶν ἐπὶ  
τὴν τήρησιν χρόνος ἐτῶν Αἰγυπτιακῶν χιθ καὶ ἡμερῶν ττε.  
Quam [Lunam] ait [Hipparchus] observasse anno  
50 tertiæ secundum Calippum periodi, Epiphi 16—  
colligitur itaque ab Epochis ad observationem tempus  
annorum Egyptiacorum 619 et dierum 315." *Lib. 5.*  
*sect. 3.*

following

following autumnal equinox: but if that 50th year had *begun* only at the summer solstice before August 5, we must then add only 49 to 152, which make only 201; and these added to 127½ before Christ (reckoned back to the summer solstice before August 5 in 128) make only 328½; and thus would place the *first* calippic year no earlier than at the summer solstice in the 329th year before Christ; which is contradicted by all other computations in Ptolemy. Hence then it follows with certainty, that if these numeric letters be correct in the text of Ptolemy, the 50th year above mentioned must have *ended* at the following autumnal equinox, and not have only *begun* at the summer solstice before August 5 in the year 128; consequently the calippic years must have been reckoned from the autumnal equinox, and not from the summer solstice. This is so demonstrably in favour of the autumnal equinox, that Peravius could no other way avoid here the conclusion against himself than by having recourse to his former practice of arbitrarily supposing the text to be corrupted, and that instead of 50 (ν) we ought to read 51 (ναι): and thus he has contrived to get rid of the *only two* proofs to be found in Ptolemy, which affect the matter in question, and are capable to decide either one way or the other; both of which passages make against him, in case the present reading of Ptolemy's text in *all* the MSS. and editions be right \*.

Wherever

\* “ *Eodem pacto corrigendus alter Ptolemæi locus, lib. 5. sect. 3, ubi Lunam ab Hipparcho observatam scribit*



Wherever indeed there is any incoherence or evident contradiction in the text of an author, either with himself elsewhere or with other antients of authority, as in the *dates* of the former of the two above-mentioned passages in Ptolemy, then there is good reason to apply a conjectural correction of a probable kind, in order to restore harmony: but without the least *evidence* or a sufficient *indication* in any author whatever, that the calippic period commenced at the solstice, thus with Petavius to make the most improbable and licentious alterations in the text, in order to force Ptolemy to say something in favour of an opinion, which he no where gives the least countenance to; this is such a wanton exercise of the art of conjecture, as can never lead to any thing but to establish error under the appearance of scientific principles. Doubt, and even ignorance, are much preferable to false pretences of knowledge, or deception. Are then such arbitrary corrections as these a sufficiently secure foundation for such an astronomer as M. de la Lande to build calculations upon concerning the length of the solar year? Yet in fact, on no better foundation than this, does the whole of his computations concerning the six autumnal equinoxes of Hipparchus depend; and indeed the characters also of Hipparchus and Ptolemy have both of them suffered by this conduct of Petavius.

Thus then I have proved, that the *only* evi-

scribit anno 50 periodi 3tiæ calippicæ——Cum Buntingho Ptolemæum scripsisse credimus 51 (va) pro 50 (v)." *Doct. temp. lib. 2. c. 19.*

dence

dence in Ptolemy, to which appeal has been hitherto made in favour of the summer solstice, viz. that concerning *ληγοντι* does really prove nothing at all either way; and that on the contrary, the *only two* passages to be found in Ptolemy, which do really affect the matter in debate, are both of them in favour of the autumnal equinox; unless we consent to alter the present text of that author, and to alter it just as Petavius fancies: consequently I have at least sufficiently justified myself for placing the commencement of the calippic period in my tables at the autumnal equinox; the whole weight of antient evidence, now extant, being in favour of *this season*, and nothing in favour of the summer solstice, except unsolid criticism, ill-founded prepossession, and such extravagant conjectures as no man can place any dependence upon. If any person can produce any *antient evidence* on the other side of the question, which has not occurred to myself, I shall be ready to give it due attention; but to make astronomic computations, or even chronologic enquiries depend upon such chimæras of the *moderns* as these conjectural corrections of Ptolemy's text, is to make science rest upon a broken reed.

It may possibly be yet suspected by some readers, that among the *many* calippic dates to be found in Ptolemy's work, there may still be some others, which affect the present question besides the two above-mentioned ones in favour of the autumnal equinox; nay, Selden, as we have seen, expressly says, that *many* are to be found there, which prove the calippic period to have begun at the solstice [Quod satis ex  
Ptolemæo

Ptolemæo *non uno* in loco liquet]: Selden's words however might rather respect the *year* (330) in which the calippic period commenced, than the *season* at which it began in that year. But however this may be, Selden and his coadjutors probably placed their confidence altogether in the authority of Petavius; whose immense work then just published had gained him universal admiration, and often indeed with good reason: yet it is this just tribute of admiration, which too often misleads and deceives us; against open enemies or ignorance distrust keeps us ever upon our guard; it is the trust we place in our friends whom we honour, which deceives us to our detriment. Now it is indeed true, that Petavius does frequently appeal for evidence not only to the word *ληγοντι*, but also to the calippic dates of some of the six autumnal equinoxes observed by Hipparchus; as for instance, after the second of the above two *proofs* quoted from Ptolemy (p. 118) he adds, "Atqui Hipparchus ipse in *altera* observatione anni 32 tertie periodi calippicæ, &c." But this appeal to the calippic date of Hipparchus's observation of the autumnal equinox in the 32d year of the 3d period is still more defective in point of evidence than his appeal to the word *ληγοντι*: for it cannot be ascertained in which, out of *two* Julian years, that observation of the equinox in the 32d calippic year was made, until it be first determined, whether the calippic years began at the solstice or equinox; this is therefore an appeal concerning what is doubtful to what is equally doubtful; it is in short arguing in a circle, by attempting to prove one fact by a second,



second, which can not be ascertained until the first be fully determined. Neither ought readers to wonder, that only *two* calippic dates are to be found in Ptolemy which affect the question, if they consider that no calippic date can be of any moment in this matter, unless the time of the celestial observation, so dated, fell between the summer solstice and autumnal equinox, as in the *two* above-mentioned cases of September 12 and August 5. For this reason the four calippic dates by Athenian months, quoted in my Tables, are of no use, but mere neutrals; and the three Macedonian dates there for the same reason can prove nothing likewise, even if we were ever so certain, that they were dated by the calippic calendar. The three observations of the vernal equinox by Hipparchus are for the same reason mere neutrals. The calippic date of the summer solstice, observed by Aristarchus, as quoted in note at p. 100. may at first sight afford some prospect of information; but on farther examination this is found to be a mere neutral likewise; because the calippic year, even though it had commenced at the solstice, yet would not have commenced until the new moon *after* Aristarchus's observation. The six autumnal equinoxes by Hipparchus may at first appear promising, and accordingly to them Petavius often does appeal for corroborative evidence; but then, as I observed above, they can in reality prove nothing either way; because they are unfortunately not dated by the æra of Nabonassar also, but *only* by calippic dates: so that we can not determine in what Julian years those observations were made, until

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we have first ascertained from what *season* of the year those dates *began* to be reckoned. Thus then, instead of Selden's *non uno in loco liquet ex Ptolemæo*, it appears, that there is not so much as a *single* proof to be found in Ptolemy, that the calippic year began at the *season* of the summer solstice; but there are *two* passages much in favour of the autumnal equinox: and with respect to the *year* there are also *several* proofs, that the calippic period began in 330, either at the solstice or equinox, and only *one* in favour of the year 331; where however there is so much discord and contradiction, that there must be some error in that passage in some or other of the *dates*; so that nothing can be concluded from such a perplexed evidence either way, upon which even chronologers, much less astronomers, can depend.

But here another difficulty presents itself; I have said, that unfortunately the six autumnal equinoxes by Hipparchus are not dated by the æra of Nabonassar; for if they had, it would have cleared away all doubts: this is indeed true, but it is also true, that Ptolemy does *afterwards* mention a second time one of those six autumnal equinoxes, and then he does annex to it its date by the æra of Philip, which is just as useful to remove those doubts as by the æra of Nabonassar. Now this autumnal equinox is dated "in the 32d year of the 3d calippic period, which (he says) was the 178th year of the æra of Phillip \*;" and if we could

depend

\* ΕΝ ΤΩ ΑΒ ΕΤΕΙ ΤΗΣ Γ ΚΑΤΑ ΚΑΛΙΠΠΟΥ ΠΕΡΙΟΔΟΥ ΕΚΕΣΤΗ ΜΗΝΑΤΟ

depend upon the integrity of Ptolemy's text here concerning the date 178, all would be consistent; it would then contain a *third*, and this a *demonstrative* proof in favour of the commencement of the calippic period at the autumnal equinox: but here unfortunately again the accuracy of this date also has been suspected by all astronomers, yet in my own opinion, however, without sufficient reason; they hav-

μνηστό μαλίστα τὴν μετοπωρινὴν ἡμερίαν ὃ Ἰππάρχος ὡς ἀκριβεστάτα τετηρημένην, καὶ ἐπιλελογισθῆαι φησιν, αὐτὴν γεγενῆσθαι τῇ γ των ἐπαγομένων του μεσονυκτιου του εἰς τὴν ὃ φερντος· καὶ ἐστὶ τὸ ἐτος ROH ἀπο τῆς Αλεξάνδρου τελευτῆς. Μετὰ δὲ ΣΠΕ ἐτὴ τῷ Γ ἐτεὶ Ἀντωνίου, ὃ ἐστὶ ΤΕΓ ἀπο τῆς Αλεξάνδρου τελευτῆς, ἡμεῖς ἐτηρήσαμεν ἀσφαλεστάτα παλιν τὴν μετοπωρινὴν ἡμερίαν γεγενῆσθαι τῇ Θ του Ἀθύρ.—Πάλιν δὲ ὃ Ἰππάρχος φησὶ τὴν ἐν τῷ προκειμένῳ ΛΒ ἐτεὶ τῆς Γ κατὰ καλιππων περιόδου Ἑαρινὴν ἡμερίαν, ἀκριβεστάτα τηρηθεῖσαν, γεγενῆσθαι τῇ κζ του Μεχίρ πρωίας· καὶ ἐστὶ τὸ ἐτος το ROH ἀπο τῆς Αλεξάνδρου τελευτῆς. Ἡμεῖς δὲ τὴν μετὰ τα ΣΠΕ ἐτὴ τῷ ΤΕΓ ἀπο τῆς Αλεξάνδρου τελευτῆς Ἑαρινὴν ἡμερίαν εὐρισκομεν γεγενῆσθαι τῇ ζ του Παχων. “ In 32 anno 3 secundum calippum periodi *autumnale equinoctium* Hipparchus exquisitissime observatum significavit, & computasse ait, quod factum fuit 3 intercalarium die in media nocte, ad quam dies 4 sequebatur; & fuit annus 178 ab Alexandri morte. Deinde post 285 annos 3 Antonini anno, qui est ab Alexandri morte 463, Nos exactissime observavimus *autumnale* rursus equinoctium 9 die mensis Athur——Rursus in predicto 32 anno 3 secundum Calippum periodi *equinoctium Vernale* ait Hipparchus exactissime observatum 27 die mensis Mechir in mane factum fuisse, & fuit annus ab Alexandri morte 178. Deinde post 285 similiter annos in 463 anno ab Alexandri morte *vernale* equinoctium Nos invenimus 7 die Pachon.” *Lib. 3. c. 2.*

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ing in this suspicion all blindly followed a hasty correction by Copernicus, and the ill founded authority of each other, down to M. de la Lande himself. It is however only in the *first* sentence, where this date 178 occurs, that there has been a suspicion of *some* error having crept into the Greek text of Ptolemy, in regard to some one or other of the three dates 178, 285, 463; for with respect to the *second* sentence, where the very same three dates occur again, they are all three allowed to be correct. The suspicion in regard to the first sentence has hitherto all along fallen upon the date 178; and yet there is no reason either obvious of itself, or pointed out by any Astronomer, why the pretended error in the text (in case there really be any error) may not just as well be in one or other of the two remaining dates 463 and 285. The vulgar proverb rightly says, "Give a dog an ill name and hang him;" this has accordingly been verified in the present example; for Copernicus conceiving, by an apparent *inconsistency* (as it seemed to him) between the above-mentioned three dates, that there must be an error in the text with respect to one or other of them, he unluckily fixed upon the date 178: and all astronomers ever since have united in a cry against this date, without having offered any *sufficient proof* to support their suspicion, and the greater part of them without having even offered *any proof* at all against it; nay, they had from the very first just as much reason to let their suspicion fall upon one or other of the two remaining dates. Or rather, they never had from the first any good reason to suspect any error  
whatever

whatever in the text; this suspicion being founded only upon a supposed *inconsistency* between those three dates in the *first* sentence, when compared with the same three dates in the *second* sentence: and yet in reality there is no such inconsistency between them as Copernicus seems to have supposed, but only a *misconception* in Copernicus himself concerning the proper sense of the plain and common Greek phrase *μετα οτε ετη* \*. The real fact then is, that there has neither been any error made by Hipparchus in his observations of the equinoxes, nor by the copyists in Ptolemy's text;

\* “ Ptolemæus assumit *autumni æquinoctium* observatum post Alexandrum anno 177—deinde subjungit idem æquinoctium a se observatum anno 3 Antonini, qui erat a morte Alexandri 463: fuerunt *inter hanc* ergo & Hipparchi observationem anni *Ægyptii* 285 dies 70 horæ 7 & 1-5 unius horæ; cum debuissent esse 71 dies & *sex horæ*.” *Copernic de Revolutionibus*, lib. 3. c. 13. “ Illud accepimus *autumni æquinoctium*, quod ab Hipparcho observatum erat tertiâ calippi periodo, anno ejus 32; qui erat a morte Alexandri *annus*, ut dictum est, *centesimus septuagesimus septimus* post diem tertium quinque intercalarium in media nocte, quam sequebatur dies quartus.” *cap.* 18. “ In effluxo ab Alexandri decessu ad Hipparchi observationem tempore sunt anni 176 dies 362 scrup. 27.” *cap.* 19. [*Norimb.* 1543]. Copernicus makes not the least remark here on the propriety of his adopting anno 177 instead of anno 178, which is in all the Greek MSS and editions: he *supposed* that there was a necessity for some correction, but he manifests no choice in reducing 178 to 177 rather than augmenting 463 to 464, which would have produced the very same effect of removing the difficulty which perplexed him; he was plainly guided by mere accident and not judgment.

but it has been Copernicus and all modern astronomers, who have now for above two centuries been in an error themselves, by their not rightly comprehending, that *μετα σπε ετη* does not *necessarily* mean, as they have all along supposed, “after the *expiration* of 285 full and complete Egyptian or retrograde years;” but that phrase may equally as well mean and generally does mean in Greek authors, only *in* and *upon* the 285th year: consequently the interval contains in the *first* sentence no more than 284 full years and some additional but indeterminate overplus days, notwithstanding that in the *second* sentence it contains 285 full years and some overplus days. For the period or precise interval expressed by that phrase, will be *longer* or *shorter*, according as it is reckoned, either *inclusively* or *exclusively* of the first and last years of the period, and according to the variation of other circumstances; as for instance, whether the interval be reckoned *from* and *to* points of time near the *beginning* or near the *end* of the first and last years; which circumstances may make nearly two whole years’ difference in the length of the *interval* between the two observations. Thus, in St. Matthew, we read “after three days will I rise again” [*μετα τρεις ημερας εγερσομαι.* 27, 63] Here the *interval* is so far from being *equal* to three full and complete days, that it does not include above one day and half; for Christ was laid in the vault on Friday evening and rose again early on Sunday morning: so that we are certain of this phrase meaning here, that Christ arose *in* and *upon* the third day, and that this sum is reckoned *both* inclusively  
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of the whole of Friday as the first day, *and also* of Sunday as the last day; but as the point of time in the first day was *late* at evening, and the point of time in the last day was *early* in the morning, therefore on account of *these circumstances* the whole interval becomes *less* than three full and complete days by above one half. But it was very possible, that under *other circumstances* the *same phrase* might have denoted an interval of *more* than three full days, that is, if St. Matthew (as Ptolemy does here) had reckoned the three days *exclusively* of the day *in* and *upon* which Christ was laid in the vault at evening; for then, in case he had not arisen before the *end* of the third day, the interval *might have* been three full days and 6 hours, *i. e.* *more* than three days. This vague Greek phrase itself therefore, as it expresses nothing more than *in general* the number of *whole years*, reckoned from the *end* of one year to the *end* of some other, and sometimes inclusively, sometimes exclusively, never can nor ever was intended by Ptolemy to point out what was the *precise interval* which intervened between the *times* of the observations made *in* those years: and this interval can be only discovered and ascertained by attending to those *other circumstances* abovementioned; such as whether the points of time, *from* which and *to* which the interval between the observations extended, happened *early* or *late* in the years or days in question. Could any one then suppose it possible, that so many able philosophers for 200 years, from Copernicus down to M. de la Lande, and in such a philosophic science as astronomy,

which boasts of being founded upon certain, mathematical and demonstrative principles, should have nevertheless read Ptolemy in so negligent a manner, as to have mistaken the meaning of a common Greek phrase; although they have professedly written whole chapters and even whole treatises to explain his reasoning, and to convict Hipparchus of inaccuracy, Ptolemy's text of corruption, and Ptolemy himself even of a want of veracity?

Thus we see how extensively error, prejudice and an abuse of scientific authority pervades even the region of philosophy itself, as well as all the other districts of knowledge, and this to such a degree, as to render its most learned, most zealous and most candid votaries often blind to the simplest and clearest truths; what then must be expected from the sophistical altercations and the notorious impositions of public disputation? Yet philosophy and disputation are the new and better guides, by which the Unitarians promise to improve christian revelation and orthodox tradition, concerning the sense of Greek words and the nature of christian tenets! This example, however, added to the many which I have produced before, proves that philosophy and science are themselves, at present, but in such a feeble state, as to stand in need of being taught by others, rather than capable of becoming such teachers to the unlearned as are to be implicitly relied on. Nay indeed, more doubts, difficulties, obscurities and obstructions daily arise from that current knowledge which philosophy pre-

pretends to have unfolded to us, than from the mysteries which the christian revelation is preposterously accused of not unfolding: and this must ever be the case, when philosophy attempts, like Phaeton, to mount into the unexplored regions of Heaven, the nature of which, eye hath not seen, neither hath it entered into the heart of man to conceive; for then it must be reasonably expected to fall again headlong, without being able to attain unto divine knowledge so much as a single step farther than the most illiterate men of the multitude. But philosophy becomes even ridiculous, when, while it assumes the pomp of that appellation, it nevertheless leaves us in as much ignorance as before under the specious garb of knowledge. It is to be wished then, that this boasted guide would apply itself more earnestly to mere earthly subjects, that it may not so often overlook or mistake clear truths, which are really within the power of our comprehension: and we need only to open many antient books, like Ptolemy, in order to see, read and understand such strange examples of daily inattention to plain truths and facts; as although they may well put philosophy to the blush, yet they will have nevertheless their use, by holding up a faithful mirror before the pride of science to afford it a more full view of its own weakness.

In the present case, for instance, concerning Ptolemy, Copernicus, as we find, erroneously conceived that the *same Greek phrase* μετα σπε ετη in two different sentences must necessarily express the *same precise interval* between the two



events there respectively mentioned; and yet nothing can be more clearly obvious, than that one of these intervals might be a whole year, and even nearly two years *longer* or *shorter* than the other, according as the events in question happened at the *beginning* or at the *end* of those same years 178 and 463; but nevertheless by their thus happening in both cases *in* those same years, the distance of the one from the other might be described in *whole* years by this same *general* numeric expression, viz. that in *both* cases they happened *in* and *upon* the 285th year *after* the former epoch. Now because Copernicus perceived, that in the *second* sentence the precise interval could not amount to less than 285 full retrograde years and 70 days more, hence he erroneously concluded that the interval must have been the same likewise in the *first* sentence; and because he found, on reckoning up the interval in the *first* sentence, that it could not amount to more than 284 full years and 70 days, in case the real date was 178; hence he erroneously concluded again, that there was an *inconsistency* between the dates in the two sentences, which proved the date in the *first* sentence 178 to be a corruption for 177: for by this pretended correction that interval would be extended to *one* whole year more, and thus become 285 full retrograde years and about 70 days, like the interval in the *second* sentence. These misconceptions then, and false conclusions of Copernicus are the sole foundation for his pretended correction of the text by his reading 177 instead of 178; in all of which errors  
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he has been implicitly followed by all other astronomers\*. Whereas the real fact is, as I have

\* Thus Christmannus (1590) says like Copernicus, "In editione Græcâ mendum est, cum scribitur æquinoctium *autumnale* ab Hipparcho observatum anno 32 periodi calippicæ tertiæ incidisse in annum a morte Alexandri 178; cum is fuerit 177; neque id mendum quisque interpretum Arabum & Latinorum animadvertit aut emendare cupivit, sed solus Copernicus vidit, & constantur asseruit illud æquinoctium *autumnale* incidisse sub finem anni 177. Quod autem ita res habeat, evidenter monstrat *intervallum* inter observationem Hipparchi & Ptolemæi *duplicem*; nam inter æquinoctium autumnale Hipparchi & Ptolemæi intercesserunt *anni Ægyptii 285 & 70 dies*." *Commentar. in Alfraganum*, p. 341.

Bullialdus accedes to the same opinion, "Númerat Ptolemæus annos 285 dies 70 *elapsos* ab Hipparchi æquinoctio autumnali ad suam observationem autumnalem—perperam igitur in codicibus & MSS legitur 178 (*πον*) cum legi debeat 177 (*πος*) perperam quoque Albategnius illud æquinoctium refert ad annum 178; in eundem errorem incidit Longomontanus." *Astron. Phil.* p. 63. *Paris* 1645. But in reality Longomontanus has been the only one of all the astronomers, who has done right by thus adhering to the text of Ptolemy; yet whether he has gone farther in the right way by placing also this equinox on its right Julian year 146 before Christ, not 147, I am ignorant. But all the others, Bunting, Scaliger, Petavius, Cary, Calvisius, Street, Riccioli, &c. have blindly adopted the opinion of Copernicus; M. De la Lande also the same. "On trouve la date 178 dans l'édition grecque de Ptolémée & dans les MSS. mais Bouilliaud *fait voir* (p. 64) qu'il faut lire 177, parceque les années calippiques commençoient au solstice d'été." *Memoire sur la Longueur de l'Année Solaire*, p. 17. As to what De la Lande says here, concerning Bouilliaud's pretended

*proofs*

have shewn, that this expression in question, *μετα ὅτε ἐτη*, and this *general* mode of computing by *whole years* the distance of one event after another, without specifying also the particular moments of time *in* those years, when those events happened, is so very vague and *indefinite* that it is impossible to collect from this expression alone what the *precise interval* was between them; neither did Ptolemy intend this numeric phrase for that purpose, but only as a popular and *general mode* of computing time sufficient for the purpose intended by it, namely, to serve as a check to ascertain more certainly the two right Egyptian years in question, 178 and 463, for if to 178 be added 285 they amount to 463: he left then the *precise interval* between the two observations *in* those two years, consisting of whole years and odd days, to be collected by readers themselves from the *materials* which he has here afforded them for this purpose; and which cannot be *less* than 284 years in *either* case, nor more than 286, yet may contain any intermediate sum of years and odd days between

*proofs* in support of the mere *assertion* of Copernicus, it may be easily shewn, that they are in fact no proofs at all; and even that out of the three offered by Bouilliaud, De la Lande has quoted with approbation the very worst. If such weak conjectures and arguments as these can be produced by one learned astronomer and approved by all others, what excellent specimens do they present of the capacity of Philosophy to reason concerning any divine truths contained in the christian revelation! It is to be hoped that metaphysical philosophers, and Unitarians are better reasoners and better skilled in Greek, or else woe be to their disciples.

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these two sums, according to the *different circumstances* of the days, at which the events happened, whether near the *beginning* of those two Egyptian years 178 and 463, or near the *end* of them. But although the above-mentioned phrase by itself is *indefinite* with respect to the precise interval, yet this interval may be *determined* by means of those other two dates 178 and 463; for these prove to a demonstration, that if any one reckons up the years and days, he cannot make more than 284 whole years and about 70 days between the two *autumnal* equinoxes in the *first* sentence; while there will be found 285 whole years and about 70 days between the two *vernal* equinoxes in the *second* sentence; of both which facts Copernicus was himself very sensible, but some how took a hasty notion into his head, as if Ptolemy had said that the intervals were precisely *equal in both* the sentences; for which opinion he had no authority afforded him by the mere phrase *μετα σπε ετη* being *the same* in both sentences, nor yet by any subsequent phrase or reasoning to be found in Ptolemy \*. Now that there actually was

\* The subsequent passage to which I chiefly allude is this, Επελαβεν ΑΡΑ ἡ ἀποκαταστασις ἐφ' ὈΛΟΙΣ Αἰγυπτιακοῖς σπε ετεσι—ο και δ και κ εγγιστα μιας ἡμερας αντι—ἡμερων οα και δ. These words immediately follow the *first* of the two passages quoted at p. 125. But I have not observed, that any writer has professedly appealed to *these words* to prove, that the intervals in the two preceeding sentences were *both* of them *equal* to 285 years; although some authors do, indeed, seem obliquely to glance at these words, as containing some proof

was *one* whole year's difference between the length of the intervals in the two sentences, will appear from the following view of the *different circumstances*, which attended the observations of Hipparchus from those attending the observations of Ptolemy. In the year 178, the *autumnal* equinox observed by Hipparchus, fell on Sept. 27, no more than two days before the end of that Egyptian year; for a new year, the 179th, began at sunrise on Sept. 29; from hence to the *beginning* of the year 463 were therefore only 284 years and two days (for 284 added to 178 amount to 462): but during these 284 years, the Egyptian new year, through want of inserting an intercalary day on every 4th year, had got back from Sept. 29 to July 20, on which day, at sunrise, the 463d year commenced; and from hence to the autumnal equinox, observed by Ptolemy, at sunrise on the following Sept. 26 in this year, were 68 days more, in all 284 years and about 70 days. Hereby it appears, that the *autumnal* equinox

proof of that point; which, however, in fact, they do not. For Ptolemy could not, in *these words*, have intended to *state*, that the 285 *whole years* [ὅλοισ] mentioned *here*, were the *precise interval* in both the above sentences; because, in fact, they are not the precise interval in either of them; the first being 284 & 70 days, *i. e.* less than 285 *whole years*, and the second being 285 and 70 days, that is, *more* than 285: so that he takes *here* 285 as the nearest *medium* in *whole years* between the above *two intervals* (both of which have fractions of days) and he only draws here a *conclusion* from the preceeding *particular statement* (as the conjunction *απα* therefore proves) in a *general way* at a *medium* and in *whole years*.

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of Hipparchus was near the *end* of the 178th year, viz. only 2 days before its end; and that of Ptolemy near the *beginning* of the year 463, viz. only 68 days after its commencement; consequently these two *autumnal equinoxes* were *nearer* to each other than the two *vernal equinoxes* in the same two years 178 and 463: for the former of these was 6 months *before* the autumnal equinox of Hipparchus, and the latter was 6 months *after* the autumnal equinox of Ptolemy; therefore they must in all be *one* whole year *farther distant* from each other than the two autumnal equinoxes in the same two years; so that the total interval was 285 years and about 70 days between the two *vernal equinoxes* in those two years. The retrogradation then of the Egyptian new year's day, during 284 years, had produced the effect of causing the *vernal* and *autumnal equinoxes* to change *their places* in their order of *precedence* in the Egyptian year; for in 178 the vernal equinox *preceded* the autumnal one, but in 463 the autumnal *preceded* the vernal equinox: this change, in the order of precedence, brought the two autumnal equinoxes of those two years to be *nearer* to each other than the two vernal equinoxes of the same two years; and they must, in course, have been necessarily twice 6 months nearer, *i. e.* one whole year. Upon the whole therefore it appears, that the misconception of a single Greek phrase by Copernicus, led him to one false conclusion, this to a second, and both together to an unnecessary, hasty and detrimental corruption of a date in Ptolemy against the authority of all MSS. and evidence; he himself indeed appeals to



none from reason or otherwise ; and those which his followers have devised for him of this kind, will be found altogether insufficient and unsolid. These errors, however, of Copernicus have so blinded all astronomers ever since, through their too implicate deference to his judgment, as to have missed all of them to place the 6 autumnal observations on the equinoxes by Hipparchus *one* Julian year *too soon* ; for by their reducing 178 to be only the 177th Egyptian year, this alteration necessarily throws back that autumnal observation on the equinox, made (as he says) in the 32d calippic year, into an earlier Julian as well as Egyptian year, than according to the later date 178 : and this, in consequence, fixes all the other five observations *one* Julian year *too soon* likewise, there being no other Egyptian date annexed to any of them (as I observed before at p. 124) except to that in the 32d calippic year ; which, if placed *too soon* in the Egyptian years, it communicates its own error to all the other *autumnal* observations. But with respect to the three *vernal* observations, these will be no way affected by the error thus introduced into the autumnal ones. The ill consequences, however, of these mistakes do not end here ; for hereby, moreover, Hipparchus has been unanimously and also petulantly accused of an unaccountable inaccuracy in regard to his observations on the *autumnal* equinoxes, and *much more* than in regard to his *vernal* ones\* : this has perplexed astronomers the

\* “ Ex Hipparchi observationibus præstantissimæ habitæ sunt *vernæ* omnes & ex *autumnalibus* quarta, quæ anno

the more, because his other celestial observations are allowed to be as accurate, as could be well expected in that age. But the real fact is, that this *greater* inaccuracy has been solely caused by the modern error of placing all his 6 autumnal equinoxes one Egyptian and Julian year *too soon*; for if each of them had been really made one whole Julian year *later* than hitherto supposed, Hipparchus must of necessity have found them (as he actually did) to fall 6 hours *later* in that following retrograde Egyptian year, than what he would have found them in the preceeding Egyptian year: accordingly this circumstance of his finding them to fall above 6 hours

anno 32 periodi calippicæ contigit." *Petav doctr. temp. lib. 4. 26.* There is also the following difference between his vernal and autumnal equinoxes, in that the former fall *too soon* and the latter *too late*; but this is rather a proof of the accuracy of Hipparchus than otherwise: because it is one necessary effect of refraction (a discovery of the moderns, totally unknown to the antients, otherwise than by its effects which perplexed them in their observations) that it makes the vernal equinoxes appear to fall *sooner* than they really do, and the autumnal equinoxes *later*, as M. De la Lande likewise thus notices, "La refraction doit en effet *retarder* les equinoxes d'automne & *avancer* ceux du printemps." p. 14: accordingly we find, that the errors in the observations of Hipparchus, chiefly correspond as they ought with these contrary effects of refraction.

But as to the remark of Petavius above, that this 4th observation of Hipparchus, in the 32d year, is preferable to the others in point of accuracy; there is not the least truth in this, although affirmed also by others: this is another of the hereditary errors of astronomers. Certainly Bullialdus and Riccioli did not think it more accurate, for they preferred the *third*.

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*later* than astronomers compute that he ought to have found them, is the very *greater* inaccuracy of which he is thus erroneously accused. Hence M. De la Lande says, "that these equinoxes of Hipparchus err a full half day from the truth"\*; the *whole* of which error he charitably imputes *here* to the effect of refraction when the sun is near the horizon, a cause of error, with which Hipparchus was unacquainted; but in the detail of his work, M. De la Lande himself proves, that the *whole* of the pretended error could not be owing to the effects of refraction; because some of the observations were made at noon, when there is no refraction, and yet these are found to be in great error as well as the rest: this circumstance, has again much perplexed astronomers, and to account for it, they have had recourse to a variety of insufficient schemes and visionary conjectures; this circumstance, however, proves that there was some other cause of error *common* to them all. And the true cause of this common *apparent* error (exclusively of what was caused by refraction) was, as I have shewn, a *real one* in modern astronomers themselves, by their having conceived that the autumnal observations were made *one* Julian year *sooner* than they really were made; which necessarily produced an *apparent* error in Hipparchus of 6 hours, that is, of one half of the 12 hours in

\* "Les equinoxes d'Hipparque, que l'auteur estimoit exacts à un quart de jour pres, ne le sont veritablement, qu'à un *demi-jour* pres, à cause de l'effet des refractions." p. 13.

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question: as to the remaining 6 hours of error, Hipparchus himself candidly acknowledges that his observations could not pretend to be exempt from error to such an amount; this remaining half of the error, therefore, might indeed be owing in part to inaccuracy or refraction, and partly to the imperfection of his instruments; but even here the moderns have committed an error again, by not making their allowance for this inaccuracy upon the right side, whereby they have again, even in this respect, made Hipparchus seem to be in another error of 6 hours, when he really is not. Thus then the *whole* of this pretended error of 12 hours is annihilated by the one way or the other; yet thus it is that even astronomers are found to deal in romance as well as Dr. Priestley and other philosophic writers; like these also they daily rack their *invention* to assign *causes* for facts and errors, before it be any way certain that such facts or errors ever existed. This is, indeed, one of the common and natural *extravaganzas* of science and philosophy; for the minds of its votaries being ever magnificently employed in searching after *causes*, they disdain the more humble task of investigating *facts*; and thus, while their heads are exalted into the clouds, their feet are too apt to stumble at a mere straw upon the surface of the earth, and then down falls philosophy. What excellent guides, therefore, must these be to our knowledge of the nature of the Deity, and how very capable must they be to teach revelation what divine truths it *ought* to have taught mankind! Hence it has come to pass,

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likewise, that one of the most able and famous astronomers of modern times, M. De la Lande, has published even a whole pamphlet in order to examine and account for the *supposed errors* of Hipparchus concerning the equinoxes; without having discovered a single link in the long chain of *real errors* made by the moderns, or obtained so much as a glimpse of any one of the above obvious truths and facts; but on the contrary, he has added many new mistakes of his own, concerning the sense of Ptolemy, to the long accustomed and hereditary errors of his astronomic predecessors; and all these errors together have misled him to another still, that is, to shorten the solar year to  $48' 48''$  instead of  $48' 53''$ ; so much does error produce error! Nay, so very wide and extensive is the influence of conjectural errors, when once become current and established as truths, that these mistakes of astronomers have communicated themselves to chronology; by adding an apparent confirmation to that erroneous supposition of another set of conjectural romancers concerning the commencement of the calippic years at the summer solstice: for if the right reading of Ptolemy's text in *both* sentences be (as all the MSS and editions exhibit it) *178 years* [con] then the necessary consequence must be, that the years of the calippic period *could not possibly* have commenced at the summer solstice; nor even at any point of time *before* the autumnal equinox, nor, indeed, until the new moons next *after* that equinox, as will appear from the sub-

subjoined considerations\*. Now that 178 [con] as in the present text, is really the right reading,

\* That the present date of 178 in the text of Ptolemy, is in favour of the commencement of the calippic year at the autumnal equinox, will easily appear from the following considerations: The 178th year of the æra of Philip is the 602d year of the æra of Nabonassar (i. e. 178 added to 424) for the last year of Alexander ended with the end of the 424th year of Nabonassar in the 324th year before Christ on Nov. 12, as may be seen in my first calippic table. Now on deducting 177 from 323 and two months before Christ (reckoned back to Nov. 12 in 324) there will remain 146 years before Christ and 2 months; so that the 178th Egyptian year began in the 147th before Chr. (which is called only 146th in the astronomic mode of reckoning employed by Pingrè and De la Lande. See *Append.* in note to p. 57, and De la Lande, p. 14) and it ended in 146: but the Egyptian new year's day had got back during these 177 years from Nov. 12, to Sept. 29; on which day the 178th year began in 147, and two days only after the equinox. The 32d calippic year of the 3d period began either at the solstice or autumnal equinox of 147 before Chr. If at the summer solstice, the autumnal equinox in that 32d year would therefore fall 2 days before the end of the Egyptian year 177 in 147 before Christ: but if that 32d calippic year did not commence until the new moon after that autumnal equinox, namely, on Oct. 12 (see 32d year in my tables) it would not end until the new moon at which the 33d year commenced, which was on Oct. 1 in 146; and the autumnal equinox in this 32d year would thus fall about Sept. 27, 2 days only before the end of the 178th Egyptian year; accordingly, the present text of Ptolemy affirms, that the autumnal equinox in the 32d calippic year did actually fall in the 178th Egyptian year, which as we see it could not have done, if the calippic



ing, is proved moreover by the same date 178 being found in Albategnius, in the quotation which he makes of this passage of Ptolemy in his *Scientia Stellarum*, as is also the other date 463 and the whole sum of years 285, yet without the least suspicion being suggested by him of any corruption having happened to any of the *dates* in this sentence \*, as Bullialdus expressly acknowledges, and De la Lande does not dispute †. But while this passage of Albategnius proves what was the *right reading* of

years began before the new moon next *after* the autumnal equinox, and not after the summer solstice. So that the observation of this autumnal equinox by Hipparchus must have been made in one Julian year *later* than Petavius and De la Lande *suppose*, viz. in the year 146, not in 147 before Christ, in the common mode of reckoning.

\* “ Ex Hipparchi observationibus illa in quam plurimum confidit, & de cujus veritate non ambigit, fuit observatio, quâ (ut ait) solem per æquinoctium *autumnale* transisse comperit anno 178 ab Alexandri morte die tertia intercalarium; post hoc Ptolemæus 285 annis Ægyptiacis transactis observavit æquinoctium *autumnale* anno tertio Antonini, quod fuit anno 463 a morte Alexandri 9 die mensis Athyr; cumque tempus, quod est *inter* duas observationes observavit 285 annos Ægyptiacos & 70 dies, &c.” *Scien. Stellar. Cap. 27.*

† For Bullialdus see note (\*) to p. 133. Observe also, that my quotation here is taken from the first translation of Albategnius, *Norimbergiæ*, 1537; but De la Lande, at p. 21, quotes the second translation, *Bonon.* 1645, whence we may conclude, by his silence on this head, that he found the same sums in the second as in the first translation.

Ptolemy's

Ptolemy's text, so far back as the 9th century, in which Albategnius lived; it proves, at the same time, that Albategnius had made the very same mistake concerning the *right sense* of that text, as has been made and retained ever since by modern astronomers: for these words, & 70 *dies*, shew, that the other phrases employed there, cannot be reconciled to any other sense, than that Albategnius conceived the *interval* between the autumnal observation of Hipparchus and that of Ptolemy to amount to 285 years and 70 days, just as the moderns do; a *sense* which stands in direct contradiction to the *date* 178. Copernicus perceived this contradiction, although Albategnius did not; but he unfortunately undertook to remove it by a wrong method, namely, by altering the *date* 178, and reducing it to 177, when nothing more was required than to alter the *sense* given to the phrase *μετὰ σπτε ετη*, which cannot possibly of itself determine of what length the *interval* was, whether only 284 years and 70 days, or 285 years and 70 days; and this *interval* can be only determined by the other concomitant *dates and circumstances*, which all conspire in favor of 284 years only and 70 days. This evidence of the Arabic text of Albategnius is much more antient than any MSS. of Ptolemy now extant; and Theon's commentary on Ptolemy is still more antient, for he lived in the 4th century; but unfortunately in his exposition of this chapter he has not quoted either of the two Egyptian dates 178 and 463, and as to 285 he only paraphrases the passage in my note p. 135: so that Theon gives us no assistance toward as-

certaining the integrity of these three *dates*, or the length of the two *intervals* between them, except the negative evidence, deriveable from his silence, against any error having subsisted in that age, either in the *reasoning* or *text* of Ptolemy. But although Theon fails in his assistance where we want it most, that is, with respect to the two *Egyptian dates*, yet he confirms however the *calippic date*, in question, to be 32, as in our present MSS. \*

Thus then I have proved the *integrity* of the date 178 in the text of Ptolemy, and shewn how insufficient the *phrase* was, which gave cause for its corruption into 177; at the same time I have pointed out what a long list of other *errors* have been necessarily created by that corruption, which have ever since deformed astronomy and perplexed astronomers; and which, notwithstanding all their attempts and conjectures, have hitherto admitted of no effectual remedy: whereas on the other hand, the above vindication of the integrity of 178, goes at once to the very root of the disorder, sets every thing immediately at rights, expels even all doubts and difficulties, together with all the other ill consequences resulting from that corruption; whereby harmony

\* Εἰτα παραγεί [Πτολεμαῖος] τὰς τετηρημένας ἡμερίας ἐξ ὧν ταῦτα συνηγάγε, καὶ φησιν, Ἐν μὲν γὰρ τῷ λβ εἶται, &c.—ἀπο δὴ τούτων τηρήσεων ἀποδείξας, ὅτι ὁ τῶν σπε τοῦ ἡλίου ἀποκαταστάσεων χρόνος ἡμέρας περιέχει, πλείους μὲν τῶν τούτων Αἰγυπτιακῶν ἐτῶν, ἡμερῶν ὦ καὶ πεταρτῶ καὶ εἰκοστῶ, ἐλάττους δὲ τῶν ἀνα τξξ ἡμερῶν καὶ τε-  
 παρτου, ἡμέρα α λείπουσι τὸ κ, ἐπάγει.—Ἡμερῶν ὦ must be an error for ἡμερῶν ο, or rather ἡμεραις ο; for λείπουσι τὸ κ we should read λείπουσι τὸ κ.



succeeds in the place of discord, both with respect to different passages in Ptolemy, and also between antient and modern astronomers in regard to the equinoxes of Hipparchus, which is a subject that has been for a long time one of the grand *desiderata* of astronomy. The *means* also of obtaining these objects, are as easy and reasonable as the *effects* are important; nothing more being required, than to preserve the text of Ptolemy unviolated by ill founded suppositions; and to substitute the most obvious *sense*, which naturally results from his present words and dates, in place of the visionary *suppositions* and pretended corrections of the text by the moderns. Finally, therefore, this vindication of the integrity of the date 178, does, in consequence (as I mentioned at first) supply a *third*, and this a *demonstrative* proof, that the calippic years did not commence sooner than the autumnal equinox, (which proof was the cause of this subject being introduced here) and it contributes, likewise, to confirm more strongly the integrity of the present text of Ptolemy in those *two* foregoing passages, from which my *two* former proofs were deduced of the commencement of the calippic years at the autumnal equinox (see p. 113 and 117) for hereby all the *three* dates and passages are found to be in perfect agreement with each other, and to require no pretended corrections whatever. Here then a question naturally arises, Is the coherence of Ptolemy's present words, dates, arrangement and reasoning to be accepted as competent evidence concerning *facts* in antiquity, in regard to which we have no other evidence? Or on the con-

trary, are we first of all to take the liberty of *supposing* certain *facts* concerning antiquity, and then to alter, strain and interpolate the *words*, *dates*, *arrangement* and *reasoning* of Ptolemy, until we *make* them capable of giving confirmation to those *supposed facts*? The answer which reason dictates seems obvious; nevertheless astronomers and chronologers have for 200 years united together in favor of this strange *ὑποθεσὶς προτερον* method; and the Unitarians are now following their example, in regard to the *fact* of original christian belief. Can any stronger proofs be required, that while philosophy pretends to take reason for its guide, it nevertheless departs from the very first principles of reason, as often as Dr. Priestley does from truth and facts? Scientific pretenders must first amend their own unsolid reasonings and absurd practices, before they attempt in their selfconceit to amend the doctrines of christianity. Physicians cure your own selves!

But still farther, there is another proof, a *presumptive* one at least, which may be urged in favor of the integrity of the date 178, and which will, in consequence, become a *fourth* proof of the commencement of the calippic years at the equinox; for every evidence will confirm the latter fact, which contributes to confirm the integrity of the date 178; it being impossible for this date to admit of the commencement of the calippic year at the solstice. Now this proof arises from the following consideration, namely, if it were ever so true (which I have shewn it is not) that there is an *inconsistency* between the present dates in the two sentences of  
Ptolemy

Ptolemy in question, and if this inconsistency were even sufficient to prove that an error had crept into *one* of the dates in the *first* sentence, yet it would still prove no more, than that there was an error in *some one or other* of the *three* dates therein contained; it would not follow hence, that the error was in the date 178 rather than in either of the two other dates 463 and 285. For the pretended *inconsistency* is, that Ptolemy has been supposed to say in *words* (though in fact he does not) that there were 285 *complete* Egyptian years and some odd days, between the *autumnal* observation of Hipparchus and that of Ptolemy; whereas the present *dates* contain an interval of only 284 complete years and some odd days: but this inconsistency may just as well be removed by *supposing* 463 to be an error in the text instead of 464; for on augmenting 463 by 1 year, the interval will be increased one year equally as well as by reducing 178 to 177: or again the very same effect will be produced by *supposing* 285 to be an error in the text instead of 284; for thus there will be no occasion at all to augment the interval by one year. If then we are thus at liberty to make *suppositions* as we please, why did Copernicus condemn the date 178 rather than either of the other *two* dates? He has himself offered no reason for this, nor yet any later astronomer; neither if they had attempted it, could they have offered any good reason; for  $\Upsilon\Xi\Delta$  might just as easily and as probably have been corrupted by the copiests into  $\Upsilon\Xi\Gamma$  as  $\text{ROZ}$  into  $\text{ROH}$ ; or again,  $\Sigma\Pi\Delta$  is as easy a corruption into  $\Sigma\Pi\text{E}$  as either of the others: there is therefore still good room  
for



for *presumption*, that the error *may not be* in ROH, since the accusation has not been brought home to this date *more* than to either of the others, even although it were ever so certain that there was an error in *some one* of the three dates. The whole evidence against the present date 178 is therefore nothing but a rash assertion by Copernicus, founded upon the arbitrary principle of *meo periculo*; and a very desperate correction indeed it has, in the event, proved to be; for in fact it has ever since led all the chronologers by the nose, and set all the astronomers together by the ears. Yet this is the conduct of your men of science, whose reasonings are said to depend upon secure, certain and mathematical principles, and who make boast of the name of philosophy! But while they thus treat us with only false-knowledge, are they not just as much blind slaves to the authority of others, and to their own prejudices, wild imaginations and romances, as the illiterate croud? How can these men pretend to instruct us concerning things in heaven, who thus daily betray their trust in regard to the privilege of common reason concerning things on earth? It may however be still objected, that there are *two other* passages to be found in Ptolemy, which will prove that *neither* of the two dates 285 & 463 can have possibly undergone any corruption, but must be perfectly correct. Now if this assertion were ever so true (which in part it is, as we shall afterwards see) yet still since neither Copernicus, Bullialdus, De la Lande, nor any other astronomer has ever noticed or appealed to these passages for the integrity of the  
the

the dates 285 and 463, how does it appear that they even knew of their existence? so that if they have been hitherto totally hidden and lost under the obscurity of Ptolemy's Greek page, those astronomers therefore are still equally inexcusable for adopting such a rash accusation of the date 178, as if those *two other* passages had never in reality existed. Nay, if they had even known of their existence, still those *two* passages would not justify their conduct, because it is equally true, that *two* passages likewise may be found in Ptolemy, which will reciprocally justify the integrity of the date 178 in its turn; and thus there is *positive* evidence extant in favor of the integrity of *all the three dates*: the *two* passages last referred to, I have already produced at p. 113 and 118; they form there my proofs of the commencement of the calippic period at the autumnal equinox, and are so totally irreconcilable with any other *supposition* than the integrity of the date 178, that Christmannus, Bunting\*, and Petavius have been forced to

\* Petavius alledges in my note to p. 120, that Bunting had corrected 50 (v) into 51 (va) before himself: this is indeed true; Bunting saw the necessity of allowing, that there was an error either in the *date* 50, or in the *opinion*, that the calippic year began at the solstice; but he enters into no enquiry, in which of those *two* the error subsisted; and seized hold of the *easiest* remedy which offered itself, that of altering the *date*, without making the least remark upon his pretended correction; as may be seen in his *Chronologia*, p. 175, *second leaf*. *Servestæ* 1590. This then instead of being a justification of Petavius, is an aggravation of his fault, by proving that he himself, as well as Bunting, were so prepossessed in

lay violent hands upon the *dates* in those *two* passages, and expel them in an arbitrary manner from the text of Ptolemy. The above objection then when thus strictly examined turns out a corroborative proof of the integrity of *all those three dates*, which have been so lavishly accused of corruption by astronomers in their paroxysms of conjectural romance; and not only rashly accused by them, but, as I have shewn, presumptuously judged, condemned and executed, without form or due trial, and without even consulting either evidence or jury. Such are the superior blessings of philosophy above the advantages of common sense in the rest of mankind! It appears then hence again, that the disagreement of the autumnal equinoxes of Hipparchus with the tables of the moderns, which has so much embarrassed astronomers, has arisen from the *false supposition* of the commencement of the calippic period at the summer solstice, and from the consequent pretended corrections of the above dates in Ptolemy, not from any *real corruptions* in his text; all those dates having subsisted there from antiquity down to the present age as stubborn witnesses to the integrity of *each other*; by their being all in discord, if we

in favour of the *common opinion*, that neither of them thought of examining, whether the error was not in the *opinion* rather than in the *date*, whenever these two contradicted each other.—Bunting has again in like manner blindly adopted the correction of Copernicus, in altering the *date* 178 into 177, without once suspecting, whether the error might not be all this time in their own *false opinion*, not in the *text* of Ptolemy, *Chronol.* p. 170, second leaf.

*sup-*



*suppose* the calippic period to have commenced at the solstice, but being, on the contrary, all in harmony, if we admit *the fact* of its commencement at the autumnal equinox; and that former *false supposition* not a single evidence can be produced in Ptolemy or elsewhere to prove, neither this latter *fact* will any antient author rise up to refute, but various passages in several antient authors will be found *indirectly* to confirm\*. Here then we have notable examples of the extravagance of those desperate

\* As one example of this I may here mention, that in the tract *de Apparentiis*, attributed to Ptolemy, it is expressly said that "Calippus made his observations and marked in his calendar, the times of the rising and setting of the constellations, according to the longitude and latitude of the Propontis; but Meto according to those of Athens and the Cyclades." In *Petavii Uranologia*, p. 93. Is not this a strong *indication* that Calippus had never calculated his calendar for Athens, but for the use of his own place of nativity or at least of residence at Cyzicum in the Propontis? Several Athenian colonies were settled there, who might chiefly employ Athenian months; and there, if any where, the calendar of Calippus might be in vulgar use. We know also, that in Asia minor, in which Cyzicum was situated, the year began at the autumnal equinox after the conquest of it by the Macedonians; and as Calippus did not institute his calendar until 3 or 4 years after the conquest of Asia minor by Alexander, he might make his year to commence at the autumnal equinox, in order to conform to the practice of the Macedonians, if indeed that was the usual time of their new year *before* their empire in Asia; or else Calippus might have fixed on that season, because it might be the usual time of the new year in Asia minor *before* the Macedonians entered it.

attempts, commonly called *conjectural emendations* in antient authors; which again prove to us their mischievous consequences, in addition to the many other examples of the same kind already exhibited: for it has been by means of these *specious conjectures* that the *real truth* has been so long hid from the eyes of the learned; namely, that the errors, which the great accuracy of modern observations had long ago found subsisting in the equinoxes observed by Hipparchus, without having been hitherto able to discover the *cause*, do in fact arise from a mistake in modern *Chronology* and not in antient *Astronomy*; as I shall afterwards more minutely confirm by an examination of the whole of what Ptolemy says upon the subject. Does chronology then terminate in mere speculation, when it is found thus necessary to remove the visions of astronomers as well as to refute the falsehoods of Unitarians?

The *harmony* therefore, which will hereby be produced between the observations on the equinoxes by Hipparchus, and those by modern astronomers, may be reasonably considered as a *fifth* proof, that the calippic years must have commenced at the autumnal equinox. For at present the *times*, of the equinoxes, as observed by Hipparchus about 150 years before Christ, and by Ptolemy 140 years after Christ, differ so much from the *times* when they ought to have happened, according to computation from the astronomic tables of the moderns, that Calvisius, Bullialdus and De la Lande do not scruple to call those of Ptolemy, *false observations*; and Calvisius affirms, that “his falsis ob-

observationibus tota astronomia turbata est"\*. Neither have the observations and character of Hipparchus himself been treated more favorably; for M. De la Lande does in effect suppose, that during the actual moments of the equinoxes, which Hipparchus pretended to have observed, he was in reality getting his dinner, or taking his afternoon's nap; and having thus negligently let the real moments of the equinoxes pass by unobserved, he afterwards set them down at random by conjecture †: with respect to Ptolemy, likewise, Bullialdus and De la Lande agree in suspecting, that he was so blinded with admiration for Hipparchus, that he has even given us an account of observations, which he had never in reality made at all, but which he arbitrarily invented in such a manner as

\* "Hipparchus observavit æquinoctium autumnale ann. 160 ante Christum, die 27 Sept. occidente sole: sed ad tabulas nostras accidit eodem die hora 7ma post mediam noctem," i. e. 11 hours sooner than according to Hipparchus. (*Opus Chronol. Calvisii apud ann. ante Christ: 160.*)—"Omnes observationes Ptolemæi integro die & amplius posteriores sunt æquinoctiis veris; sed præcul dubio deceptus fuit a suis instrumentis; nihilominus his falsis observationibus tota astronomia turbata est." *Ibid. post Christ. ann. 140.* "Liquet sane perexiguam vel potius nullam Hipparchi observationibus fidem de æquinoctiorum locis deberi." *Lydiat de annorum variis formis, p. 57. Londin. 1605.*

† "Il y a donc apparence, que cette fois là Hipparque ne fut pas très attentif pendant le cour de la jour née; peut-être qu'ayant vu le matin, qu'equinoxe ne fut venu, on négligea d'y regarder à midi, & le soir on vit l'equinoxe trop tard." *De la Lande Memoire, p. 14.*

should



should give confirmation to the more antient observations and opinions of Hipparchus\*. These are heavy accusations, and so much the

\* “Cum igitur Ptolemæus tam *securé* acquievit Hipparcho, qui in anni definitione *errore* non vacat, observationes Ptolemæi quæ Hipparchi inventis *accommodatæ sunt*, sine veritatis detrimento et citra contemptum viri tam excellentis, dimitti possunt.” *Bulialdi Astron. Philolaica, lib. 2. p. 64.*

“Satis apparet Ptolemæum noluisse immutare Hipparchi hypothesim solarem—Ptolemæi observatæ equinoctia admittere non possumus salvis aliis; nam incidemus in annuam inæqualitatem, cui repugnant omnes observationes a nobis adhibitæ—illi veniâ digni sunt, qui Ptolemæi observationes *in dubium* revocare *verentur*; tantum enim ducem sequentes, pari studio erga illum tenentur, quo ipse Hipparchum persecutus est: verum tamen in offensionem cujusquam incurrere haud debet, qui Ptolemæum ostendit timidè nimis sibi que nimium diffidentem, Hipparcho tam sanctè adhæsisse in definiendo annuo solis cursu.” *p. 70, 73.*

“Ptolemæus preoccupatus determinatione Hipparchi quantitatis anni, *obtrusit* nobis sua æquinoctia *uno die serius facta*, quam *ipse observavit*.” *p. 74.*

“Les 3 equinoxes de Ptolémée des années 132, 139 & 140 (apr. Chr.) ne s'accordent point du tout avec ceux d'Hipparque: c'est ce que l'on a remarqué bien de fois, spécialement Bouillard, qui les a rejettés dans ses recherches sur la theorie du soleil,” *De la Lande Memoir, &c. p. 21.*

“Ptolémée, rempli d'admiration pour Hipparque ne crut pas pouvoir mieux faire que de s'en tenir aux determinations d'Hipparque: mais pourquoi *faire semblant* de les avoir trouvée par ses propres observations? C'est un reproche qu'on lui fera dans tous les tems, comme d'avoir *changé les tems* des observations pour les accorder avec ses hypotheses. *p. 12.*

more

more unjust, as we find, that both Hipparchus and Ptolemy may be exempted from pleading guilty to the most severe part of these judgments, by only retorting upon these moderns the very same accusations; namely, that they themselves have all dealt so much in mere conjectures, and have been all so blinded *with admiration* for the opinions of Scaliger and Petavius concerning the commencement of the calippic year at the summer solstice in Athens, as even publicly to appeal to Ptolemy for proofs of that fact; although in truth no such proofs were to be found in his own genuine work, before it was adulterated by the pretended corrections of the moderns, nor yet in any other antient author now extant: and moreover, these moderns have been also such downright copieists, as without examination to follow Copernicus in his hasty and merely accidental correction of the text of Ptolemy concerning the *date* 178, when by preserving the original date of Ptolemy, they would have been able to have freed both Hipparchus and Ptolemy from the chief part of these unjust accusations; and they would in consequence have even found, that the observations of both those antients correspond as nearly with the tables of modern astronomers, as could be reasonably expected, considering the imperfection in regard to instruments, and the defective method of making observations on the equinoxes in those early ages of science. So that in fact it is the moderns themselves, who have built upon a *false foundation*, namely, upon the literary prepossessions and learned romances of modern *chronologers*, who in their turn have been reciprocally misled

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by pretended corrections, but real corruptions in the text of Ptolemy, made by Copernicus and other modern *astronomers*. Par nobile fratrum! It is not however so much their inattention, much less their want of erudition or ability, which I accuse, as their indiscretion, in having placed too much dependence upon their own ingenuity, and hence suffered their conjectural hobby horse to run away with them, until they have all become lost in a wilderness of error and supposition. Yet thus extravagantly some learned men go on to write and reason, and thus other learned men for whole centuries blindly copy the errors of the former with approbation, although against all reason; and thus also rash correctors of the text of ancient authors still go on, by creating errors where there were none before, and where their pretended corrections lead likewise to eternal discord, and to nothing but hyperborean darkness: whereas on the contrary, when *harmonious consequences* follow from any opinion, supposition or conjectural correction, as in the present case from the *opinion*, that the calipic years began at the autumnal equinox, these must in all reason be admitted as *corroborative*, although indeed *indirect proofs* in favour of such opinions or corrections, in case we are in distress for other and more *positive* evidence; for *harmony* must be ever the natural effect of truth, although it may possibly, in some cases, by an extraordinary accident, subsist even without truth. This *conspiring evidence* then of different passages of the present text in favour of 178, that *discord*, which has arisen between different passages of the same text, by its being altered to



177; and this *harmony* (in case we preserve the present reading 178) which will be hereby produced between the equinoxes of Hipparchus and Ptolemy compared with the modern tables, between which there is nothing but discord at present; these circumstances together must be reasonably admitted as a *fifth* proof, that the calippic year began at the autumnal equinox, not at the solstice: and it is a proof also, which is again deduced from the contents of Ptolemy's work, in which *so many* evidences are said to exist to the contrary; yet where in reality we find not a single one, but *all* of them in favour of the autumnal equinox, or else mere neutrals.

A *sixth* evidence, that the calippic year began at the autumnal equinox, may be deduced from the following review of the circumstances relative to the *five* foregoing proofs; for if the state of Ptolemy's text, in regard to *the dates* in the above mentioned *three* quotations, was originally such as De la Lande and other moderns conjecture; and if it has really suffered in *those three* places such revolutions as they pretend, these are certainly as extraordinary, wonderful and romantic events, as any which are to be found among the achievements of Amadis de Gaul and Don Quixot. For since there are so many calippic dates in Ptolemy, which are mere neutrals, and can prove nothing either way, what can be more extraordinary, than that *those two* calippic dates and *that one* Egyptian, which do affect the present question concerning the commencement of the calippic year, should have been corrupted by the copiests, and *those*

*alone?* At least I know of no other calippic dates, concerning which any such calamity has been suspected to have happened: \* But it is still ten times more extraordinary, that *those three* dates *alone* of years should not only be corrupted, but also corrupted in such a manner, that instead of being thereby turned into mere neutrals, or else rendered totally anomalous and inconsistent with all other dates by years found in Ptolemy, they should on the contrary have been *all three* so corrupted as to be turned into foes instead of being any longer friends; and should become evidence after their corruption in favour of the autumnal equinox, notwithstanding they were before their corruption evidence for the summer solstice; thus wonderfully do they happen to change sides from the treasury bench directly into the arms of opposition! When the *only three* passages extant in antient authors, which were able to decide the point in question, had thus turned their back on the opinion of Petavius concerning the summer solstice, this circumstance made it at least highly expedient for M. de la Lande to have fairly informed his readers in what manner Petavius and himself had undermined the æquinoctial castle; and by what antidotes and intoxicating potions they had charmed asleep the dragons, which defended it, in order to render the text

\* The errors in Ptolemy's text, are chiefly concerning *days* of months or sums of *days*: I know of only one corrupted Egyptian date of a *year* in the 1st edition 1538, and this may possibly be corrected by MSS. in the later editions, and be a mere error of the press.

more propitious to the prepossession of modern chronologers and astronomers in favour of the summer solstice: for a very moderate knowledge of the dangerous nature of those antidotes, would have made many persons allow, that they might as well have permitted Ptolemy to die at once, as have attempted to cure his text by such desperate medicines. In fine, if after all, the calippic year did really commence at the summer solstice, it must at least be confessed, that it has had most abominable ill luck or ill treatment; for not only Ptolemy himself by his brevity, but also all the copiests of the middle ages by their negligence, and even old Time himself, by means of his ancient evidences and manuscript authorities, must have all joined together in a combination and wicked conspiracy against the poor summer solstice, to deprive it of the honour of giving origin to the period of Calippus. Upon the whole then, am I myself the *Pere Hardouin* (as Dr. Priestley unjustly alledges) who has condemned almost all antient authors as being the forgeries of later ages? or is it not rather your Petaüs, your Priestleys, Bouillauds and De la Landes, who first interpolate the antients with forgeries of their own, or make their text say just whatever these moderns please; and then condemn the antients as negligent blunderers, because their original text will not countenance the chimeras of these interpolaters, until after it has been new manufactured by the moderns agreeably to their own taste? Is this a conduct according to knowledge, evidence, and judicious criticism? or rather have not all these con-



derations been overwhelmed by the whirlwind of caprice and the specious visions of mere opinion, taken up at first in haste, supported afterwards with obstinacy and with all the licentiousness of improbable conjecture?

There is however still another passage in Ptolemy, which at first afforded me some hope of its supplying evidence either to confirm or refute the foregoing *six* proofs; but on farther examination, this likewise turned out to be a mere neutral, which decided nothing; I shall nevertheless point out the passage, both because it has not been referred to by any writer, and also because it will hereby be at least taken out of the hands of those, who may otherwise just as well employ this passage as the above-mentioned ones for the purpose of proving the commencement of the calippic period at the summer solstice: and I notice it moreover for this additional reason, because it will confirm still farther the abominable ill luck, which has throughout attended this subject and all these passages of Ptolemy in question; either by their being suspected just when their testimony is of most consequence, or else by thus turning out mere neutrals, where they might have become *demonstrative* testimonies in favour of the one opinion or the other.

Now Hipparchus finding that he could not determine the true length of the solar year, by making observations on the equinoxes, on account of their being subject to an error of 6 hours (which was an error great enough to set aside all conclusions on that subject, during so short a period as the 33 years which he employed

ployed to that purpose, but was of no such importance in the longer period of 285 years employed by Ptolemy) he had therefore recourse to another method, namely, by observing the different distances of some conspicuous fixed stars from the autumnal equinoctial point in the heavens during different lunar eclipses, which happened *near to* that point.\*

This method however was, as Ptolemy observes, only exchanging one doubtful method for another equally or more doubtful; but this is foreign from our subject, which has no concern except with the dates of his observations during those lunar eclipses, “one of which was made in the 32d calippic year and an other in the 43d.”† Now although he has not mentioned the months or days in those years, when the observations were made, yet by the circumstances of their being made during lunar eclipses, which happened *near to* the autumnal equinoctial point and to the star *Spica*, which is in the constellation *Virgo*, corresponding to our September; we are hence enabled to determine on what Julian days the observations must have been made in those two years. The 32d ca-

\* Επιλογίζεται [Ιππαρχος] δια τινων ΣΥΝΕΓΓΥΣ απλανων αστερων τετηρημενων σεληνιακων εκλειψεων, ποσον καθ' εκαστην ο καλουμενος Σταχυς προηγεται του μετοπωρινου σημειου. *Lib. 3, Sect. 2.*

† Εκ της εν τω ΑΒ ετει της τριτης κατα καλιππον περιόδου παρατεθειμενης εκλεπτικης τηρησεως ευρισκειν οιεται [Ιππαρχος] τον Σταχυν προηγουμενον του μετοπωρινου σημειου μοιρων ε, ε, δια δε της εν τω Μ και Γ ετει της αυτης περιόδου, προηγουμενον μοιρων ε, δ. *Ibid.*

lippic year of the 3d period began in the year 147 before Christ (either at the summer solstice or autumnal equinox) and ended in 146; and there was a partial lunar eclipse in 147, on Oct. 25, but a total one in 146, on April 21\*; both these fell *in* the 32d calippic year, but unfortunately neither of them happened between the summer solstice and autumnal equinox in *either* of those two years, therefore they are both neutrals, and prove nothing concerning the time of the commencement of the calippic years. The total eclipse on April 21, must have been that during which Hipparchus made his observation; the moon, when in opposition to the sun, being then *near to* the autumnal equinoctial point and the star *Spica*. The 43d calippic year began in the 136th before Christ (either at the summer solstice or autumnal equinox) and ended in the 135th. Now there was a partial eclipse of the moon on Sept. 24 in 136, and a total one on March 21 in 135†: the latter of the two could alone have been the eclipse in question, as the moon then in opposition would have been *near to* the star *Spica* and the autumnal equinoctial point. Here therefore we see the ill luck attending this subject; for the date

\* “Ann. av. Chr. 146, eclips. part. de lune, Oct. 25, a 5½h. soir.—Ann. 145, eclips. tot. de lune, Avril 21, a 9¾h. soir.” *Chron. des Eclip. par M. Pingrè, Hist. de l’Acad. des Inscript. tom. 42.*” N. B. M. Pingrè’s ann. 146 is our 147.

† Ann. 135, eclips. part. de lune, Sept. 24 a 4½h. soir. Ann. 134, eclips. tot. de lune, Mars 21 a ½h. matin. *Ibid.*



of the former eclipse on Sept. 24 would have fallen between the summer solstice and the autumnal equinox, therefore would have proved whether the 43d calippic year had commenced *before* the new moon next after that equinox in 136: but for Hipparchus to have made his observations at this partial eclipse, it must have been in regard to some star *near* to the *vernal* equinoctial point, and not the star *Spica* near the *autumnal* equinoctial point; so that by his unfortunately making his observations on the star *Spica* and the autumnal equinoctial point, not the vernal, these dates turn out again to be both of them mere neutrals. There was also another and a total lunar eclipse in the year 135, which would have served the same good purpose, namely on Sept. 14, in case Hipparchus had happened to have made his observations on a star *near* to the *vernal* equinoctial point; for Sept. 14 falls again between the solstice and equinox. Thus we have nothing except ill luck; which however proves the more strongly how cautious we ought to be in altering by conjecture and without any sufficient cause the *only three* dates in Ptolemy, which do enable us to ascertain the matter in question.

Thus then it appears, that in astronomy, as well as in the many other parts of literature displayed already, the most able of the moderns are sometimes *all in the wrong* themselves, while they are pouring out their complaints against the negligence, inaccuracy and even want of veracity in the antients. Is there not good reason therefore to suspect, that the invectives of our philosophic Unitarians against the originality

nality and rationality of the orthodox tenets of christianity, may have no better foundation than some similar mistakes of the Unitarians themselves? And if so many veins of weakness, inattention, mistake and visionary conjecture are thus daily found to pervade all branches of human knowledge, where is that pretended privilege, which philosophy assumes, of being able to purge from error the visual ray of our understandings? The doctrines of christianity seem indeed to stand no chance of receiving any great improvement by appealing from the infallibility of the Pope to the infallibility of philosophy. Nevertheless our Unitarian reformers are now acting the very same part by the *sense* of the Gospel, as the above-mentioned scientific reformers have acted by the *text* and *sense* of Ptolemy; and the consequence must be the same, that they will render the sense of the Gospel as incoherent and chimerical, as Ptolemy's correctors have rendered the mangled carcase of his Greek text: both set of operators seem indeed determined to have no mercy on any thing which stands in their way; but are both equally ready to expel, with high and dogmatic authority, whatever *words* or *meanings* they find to be not commodious and favorable to their own theoretic systems. Is philosophy then a fit judge of divine subjects, which is thus repeatedly found unable to judge even itself and its own earthly concerns? And what security against error can be derived from such a feeble guide to oppose to the written or traditional truths taught to us by revelation, and received almost universally during so many centuries?

But

But at least we may well say with St. Paul, "Oh! wretched men that we are, to be thus beset with so many dangers and difficulties on every side!" For both too much ingenuity and also too little, both too much reliance on antient opinions and also on modern innovations, both philosophic and also popular conceptions, both learning and ignorance, all seem equally disposed in their several turns to mislead and deceive us! What Winkelman observes concerning dissertations on the *arts* of the antients, is therefore but too true also in the learned and *philosophical enquiries* of the moderns, "that they are often made with so little reflexion, and founded on such weak conjectures, as to be more capable of perplexing than instructing their readers.\*" But as Dr Priestley conceives that he is possessed of a felicity in discovering the mysteries of antiquity, I called upon him lately to try his dexterity on a particular subject there pointed out, yet hitherto however he has been silent concerning that difficulty†: I call upon him now again to come and see the nakedness of the land of modern philosophy; to come and see how scientific men themselves have forsaken the plain words and meaning of Ptolemy, in order to pay a slavish obedience to the authority and mistakes of each other during two whole centuries, in the most enlightened and philosophic age of the world. It were to be wished,

\* "Elies sont faites avec si peu de reflexion, qu'elles sont plus capables de nous egarer que de nous instruire." *Preface au Hist. de l'Art de l'Antiquité.* p. 7.

† Crit. Obs. Append. vol. 3. p. 47

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therefore, that the view of this may induce him to preach another *sermon* to the public; yet not, as before, upon the benefits of *free philosophical enquiry*, but upon the advantages of *fair, rational enquiry*: and instead of his former text, “he that hath ears to hear, let him hear the erroneous and extravagant opinions of other men;” a more useful subject would be, “he that hath eyes to see, let him see his own errors and correct them,” together with the many false principles, which deform almost every branch of modern knowledge, and even those branches no less than others, where *freedom of enquiry* has been ever exercised in the most ample manner; for indeed the defects of literature in the present age do not arise from a want of freedom in enquiry, but from a want of due attention and of a candid rational enquiry\*. Let Dr. Priestley come then and see the *baseless fabric*, upon which the most skilful astronomers of mo-

\* The following passage in the *Dissert. on Job*, by *Peters*, is very apposite to the present subject, “*M. le Clerc*, a name, which carries with it the greater authority on account of the professed *freedom of his enquiries* (which I am far from discommending) is a remarkable instance how much a *free enquirer* may be enslaved to a particular notion; which groundlessly taken up and obstinately pursued, may run him upon a train of blunders and mistakes. This learned man having taken it into his head that a future state was unknown to the Hebrews, has with all his critical sagacity and good skill too in that language, overlooked, perverted and strained a hundred places, which would have stared him in the face with it, had not this strong prejudice made him turn away his eyes, and fix them upon any thing rather than this.” *Introduct. p. 8.*

dern times have erected some of their *astronomic tenets*; and the equally tottering superstructure, which M. de la Lande, notwithstanding all his great abilities and *free speeches*, has built upon that false foundation; after this let him seriously ask himself, what is secure from human infirmity? Alas, man's weakness loosens the foundation of every noble work! Neither philosophy any more than religion is able to say with certainty, *exegi monumentum ære perennius*. If therefore in the abovementioned *historic* and *astronomic* subjects of *free enquiry*, Dr. Priestley finds it difficult to reconcile the tenor of history in the antient gospels with chronology and astronomy, or again to reconcile modern astronomy with the antient Greek of Ptolemy; why should it be thought strange, if it be found equally difficult to reconcile the doubtful acquisitions of knowledge, which the pride of science dignifies with the name of philosophy, to the verbal information given us by revelation concerning the peculiar nature of the divine being, so different doubtless from all terrestrial objects of knowledge? But as soon as *fair, rational enquiry* shall in any of these subjects have dispelled the fanciful conjectures of the moderns, and detected the intricate arts of imposition employed to support them, how quickly then will the shades of error flee away before the dawning light of truth! Then verily the words of the Prophet will be once more fulfilled, "see—it turneth their wise men backward, and maketh their boasted knowledge to become like foolishness; it frustrateth the dreams of conjectural romancers, and maketh their *diviners* appear as mad."

mad." *Isaiah* xliv. 25.\* In the mean time amidst this deluge of learned errors, systematic speculations and extravagant visions, which daily lay claim to the name of philosophy, I myself have not been wanting along with the zealous prophet "to lift up my voice on high, and cry to the watchmen of the city of Truth, to resist this muddy torrent which has broke in upon us; I have called aloud and spared not;" I have repeatedly protested against this abuse of the name of philosophy, and against the many chimæras which assume that pompous appellation; I have exclaimed again and again *quo rueris miseri? Heu! scribimus indocti doctique deliria passim.* It was therefore with too much reason, that St. Paul long ago exhorted christians to beware of the deceitfulness of vain philosophy, and the wrangling spirit of disputation. But do we find that this admonition has produced its proper effects? On the contrary, does not philosophy even take to itself the merit of those grains of reason, and those religious virtues, which the spirit of christianity has now at length gradually diffused among all nations? While in reality philosophy still retains its own former habit of intricate and deceitful, although often specious and ingenious chicanery. Is not the caution then afforded us by the above specimen concerning Hipparchus and Ptolemy, more necessary now than ever, in order to guard christian readers against the

\* It is accordingly said, that a certain writer has already obtained, among the Dissenters, the name of the *Spiritual Quixot.*



snares of plausible but unsolid argumentation, hid under the garb of learning and recommended by the honors of scientific academies? From the sufferings of Ptolemy under the hands of chronologers and astronomers, we may conclude what the sacred scripture must equally suffer from the pens of metaphysical Unitarians; and how easy it is under pretence of *freedom of enquiry* to distort the sense of its words in such a manner, as to make it favor their own prejudices; just as we see is thus practised concerning prophane authors of antient times. Upon the whole then, what a poor affair is this boasted human philosophy, whether physical or metaphysical! Instead of clearing our minds from error, it daily employs the appearance of science and reason as its instruments to confirm us the more in bondage to ignorance; it is not even secure from such gross mistakes concerning the sense of antient words as school-boys would be ashamed of, any more than from such infidelities and contradictions in reasoning as men of no learning ought to blush at; of all which and many more infirmities, I have exhibited such numerous examples from Dr. Priestley's philosophic theology in my *Appendix to Vol. 4.* as will do no credit to the F. R. S. or to the long list of important capitals which he has added to them. Not but real philosophy is, nevertheless, a very desirable acquisition; for what indeed is it except the love of truth and the right exercise of reason? Only those writers then are the objects of the Apostle's condemnation, who abuse the solemn name of philosophy, in order to im-  
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pose upon unwary readers with disputatious chicanery, without either truth or reason; and who under the pompous appearance of learning only give vent to the wild dreams and extravagancies of their own imaginations: thus instead of *historic truth* and *philosophic argument*, they present us with such an indigested mass of speculative and metaphysical crudities, as would even disgrace a dissertation on philosophic fireworks or philosophical razors.

After having thus collected and connected together the *several proofs*, which are *really to be found* in Ptolemy concerning the commencement of the calippic years, and which are all in favor of the autumnal equinox (nay, it appears from my note at p. 143, that they could not have begun until *after* that equinox, therefore probably, *in general*, at each new moon after the autumnal equinox) it still remains to explain and confirm, more at large, my *fifth proof* concerning the *harmonious effects*, which will follow from this, toward reconciling the dates of the observations on the equinoxes by Hipparchus with the dates of the same in the same years, as exhibited by the astronomic tables of the moderns. Now the necessary consequence of the commencement of the calippic years at the autumnal equinox will be, that each of the 6 autumnal observations by Hipparchus must have been made *one* Julian as well as Egyptian year *later* than the moderns have determined; for if the 32d calippic year above mentioned began at the summer solstice in the 147th year before Christ, the autumnal equinox *in* that 32d year would fall near *the beginning* of it, *i. e.* within  
3 months

3 months after its commencement, and *in* the Julian year 147 : but if that 32d calippic year did not commence until the new moon *next after* the autumnal equinox in 147, then the autumnal equinox *in* that 32d year would not fall until near *the end* of that calippic year, and *in* the Julian year 146. It would be the same in regard to *all* the other 5 autumnal observations, *in case* those 5 calippic years *all* commenced *after* the autumnal equinoxes instead of the summer solstices ; and *if* the *order* of intercalary years adopted in my calippic tables be the right one, in *all* those 6 calippic years *the same* Athenian and Macedonian months would in fact all commence not until *after* the autumnal equinoxes : that *order* of intercalation is moreover, as I conceive, the only *possible one*, whereby the same Athenian and Macedonian months can be thus made *always* to commence *after* the autumnal equinoxes ; hence then seems to arise *another presumptive* proof of its being the right method. This alteration, however, in regard to the Julian years, in which the 6 autumnal observations were made, will produce no alteration of the Julian years, in which the 3 vernal observations of Hipparchus fell : for the only difference caused hereby will be, that whereas at present according to modern astronomers, the autumnal observations of Hipparchus *preceeded* his vernal ones made in the same calippic years, it will by this alteration follow, that in reality his vernal observations *preceeded* his autumnal ones dated by *the same* calippic years ; and thus the latter were made *one* Julian year *later* than hitherto supposed. It is indeed true, that Ptolemy quotes

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an account of the dates of the autumnal observations of Hipparchus *before* his account of the vernal ones; but no conclusion can be safely deduced hence, that the order of their *precedence* corresponded with the order of their *recital* in Ptolemy; the order of recital there, if not produced by mere accident, might arise from other causes, such as that Hipparchus made more in number and more elaborate observations on the autumnal equinoxes than on the vernal ones: and this might arise from what I have mentioned above concerning his observations on the different distances of the star *Spica* from the autumnal equinoctial point, during lunar eclipses; to which he might have been led again by mere accident, or because Timocharis had left behind him an account of some observations on the same star *Spica* and its distance from the summer solstice and autumnal equinox, as may be seen in Ptolemy, *lib. 7. c. 3.*

But the necessary consequence, which follows from those 6 autumnal observations being all made *one* Julian year *later* than hitherto supposed, will be, that Hipparchus must have also necessarily found the autumnal equinoxes by his observations, to have fallen 6 *hours later* than modern astronomers have calculated; for in a year like the Egyptian, which was not intercalated when the odd 6 hours had amounted to 1 day, the equinox must in every year fall 6 hours *later* than in the preceding year. Now those autumnal observations are accused by M. de la Lande of placing the equinoxes 12 hours *too late*; but when the above-mentioned mistake of the moderns concerning 6 out of those 12  
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be deducted, the errors of Hipparchus in his observations will be reduced to 6 hours only; and he himself acknowledges, that he found himself not able to ensure his observations from being liable to an error of 6 hours: the cause of which, although he was ignorant of it, as is now known, might have been a deception arising from refraction; just as when a shilling is put into a basin and hid from a spectator's eyes by the edge of the basin; if you pour water into the basin, he will see the shilling, which he could not see before. It is rather wonderful then that Hipparchus was not in a still greater error than of 6 hours, since M. de la Lande calculates, that at the moment of sun-set and sun-rise this cause may produce an error of 16 hours\*. This error then of 6 hours ceases to be an error, when thus pointed out to us both by Hipparchus and Ptolemy, and when we are cautioned by them to guard against it; and even this error will not be always on the same side, by the autumnal observations being all fixed by him *too late*, as the moderns affirm; but it will be sometimes on the other side, by their being fixed *too soon*, and this even in his autumnal observations as well as vernal ones: neither will it amount to so much as 6 hours except in 1 or 2 cases; so that upon the whole the observations of Hipparchus will hence appear to have been made with as much accuracy as could be expected in that age†. Here then

\* "La refraction sous la latitude d'Alexandrie—ferait 16 heures de retard." p. 14.

† The observations of Ptolemy are not however defensible by the same principles of vindication as those of  
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I have to lament again, just as upon so many other occasions before, that I am continually obliged to fight my way through a host of foes, false accusations and difficulties, raised and maintained by the united efforts of different classes of scientific men; who however they may have dissented from each other in many respects, yet in this one point they have agreed, to support a numerous body of ill-founded *conjectures*, hasty *opinions*, strange specimens of *negligence*, untrue *assertions*, unsolid *reasonings*, unjust *accusations* and insufficient *authorities*, all of which they have placed in my way as janizaries to defend the castle of error and to blind the cause of truth. Such are the unhappy effects of warm disputation, and the want of sober, rational en-

Hipparchus, but they may be equally justified by another principle, which is a consequence of the preceeding ones. For Ptolemy finding, on account of this error of 6 hours *and more*, that he could not ascertain, when the precise moment of the equinox *was arrived*, he has therefore set down the *earliest hour* at which he could determine that it *was actually past*; now this he could determine *with certainty*, without misleading his readers with *doubtful* observations, agreeably to what he professes repeatedly, viz. not to set down any observations except such as he could ascertain with *indubitable certainty* [μαλιστα ἀδιστακτως τετηρημένας]: and there would still remain *difference enough* between 285 solar years and 285 Julian years for Ptolemy to prove what he had in view, viz. that the solar year was shorter than the Julian year by at least the 300th part of a day. This Hipparchus had *conjectured* and *contended* for, but it was reserved for Ptolemy to *prove* it by *indubitable* observations: and yet he is abused by astronomers because he did not go farther than he could.

quiry!



quiry! The case of Dr. Priestley, we see, is but too common a case in other subjects, even of a philosophic nature; for it is not, as is idly alledged, merely in theology, that learned sophistry and want of candor are to be found: these defects are just as frequent in all other parts of knowledge; and if they have been erroneously considered as more peculiarly inherent in theology, this has happened for no other reason, than because it is a subject, which all ranks of men think themselves wise enough to understand, and learned enough to determine whether it be treated of with judgment or not.

Let us therefore now produce at length those two principal passages of Ptolemy, which relate to the present matter in debate; in the *first* of which Ptolemy gives us an abstract of the observations made by Hipparchus on the equinoxes, and in the *second* an account of his own observations on them, together with a comparison of his own with those of Hipparchus, and the necessary conclusions which follow thence. Hereby my readers will have an opportunity of seeing the whole connected together; and therefore will be better able to judge, whether the words and reasoning of the author have not been too little attended to by those who have been the most forward and severe in their accusations both of Hipparchus and Ptolemy.

Παρατίθεται πρῶτον [Ἰππάρχος] μετοπωρινῶν ἰσημεριῶν χρόνον, ὡς ἀκριβεστάτα τετηρημένων,

I. Ἐν μὲν τῷ ΙΖ εἰτε τῆς τρίτης κατὰ καλιππον περιόδου, τοῦ Μεσορῆ Α περι τὴν δυσιν τοῦ ἡλίου.

II. Μετὰ δὲ Γ εἰτε ἐν τῷ Κ εἰτε, τὴν νεωμηνίαν τῶν ἐπαγομένων, πρῶτης, δευρ τῆς μεσημβρινῆς· ὥστε διαπεφωνηκεναι Δμίας ἡμέρας.

III. Μετα δε ενιαυτον εν τῷ ΚΑ ετει ὥρα γ, ὅπερ και ην ακολουθον τη προ αυτης τηρησει.

IV. Μετα δε ΙΑ ετη τῷ ΛΒ ετει της Γ των επαγομενων εις την Δ, του μεσονυκτιου, δεον πρωιας· ὥστε τῷ Δ παλιν διαπεφωνηκεναι.

V. Μετα δε ενιαυτον ενα τῷ ΛΓ ενιαυτῷ τη Δ των επαγομενων πρωιας, ὅπερ και ην ακολουθον τη προ αυτης τηρησει. \*

VI. Μετα δε Γ ετη τῷ Λγ ετει, τη τεταρτη των επαγομενων, εσπερας, δεον του μεσονυκτιου· ὡς τῷ Δ μονῷ παλιν διαπεφωνηκεναι.

Μετα δε ταυτα εκτιθεται και τας ὁμοιως ακριβως τετηρημενας εαρινας ισημεριας.

I. Εν μεν τῷ ΛΒ ετει της τριτης κατα καλιππον περιόδου, Μεxερ ΚΖ, πρωιας, και ὁ κρικος δε φησιν ὁ εν Αλεξανδρειαισιν εξ ἐκατέρου μερους παρηνυγασθη περι Ε ὥραν· ὥστε ἤδη και

\* The edition of 1538 reads τῷ ΛΔ ενιαυτῷ, τη ΛΓ. This τη ΛΓ must certainly be an error: and instead of τῷ ΛΔ Bullialdus reads ΛΓ, he pretends also to have consulted the MSS in the King's library at Paris (see *Astron. Philo.* p. 61): he is probably right, and the figures ΛΔ seem to have changed place with ΛΓ. However, if the MSS do not confirm the reading ΛΓ ενιαυτῷ, it ought to be observed, that though Ptolemy makes only *one year's interval* between ΛΒ (32) and ΛΔ (34) it does not necessarily follow that the latter is an error for ΛΓ: for if the 33d year began at the moon *after* the equinox, and the 34th year *before* it, there would be an interval between the equinoxes of 32 and 34 of only *one full year*. The MSS therefore ought to be minutely consulted concerning this reading, and we ought not to depend upon our own conjectures of what is right. The metonic years began at the new moons, sometimes *before* and sometimes *after* the solstice, and so might the calippic years in regard to the equinox. If then the MSS should confirm that ΛΔ is the right reading, this would be a *demonstrative* proof, that the calippic years began at the equinox: it might however follow hence, that the order of intercalary years in my tables is not the right one.

την αὐτὴν ἰσημερίαν διαφορῶς τετηρημένην Ε ὥραις ἐγγίστα διέ  
νεγκειν\* καὶ τὰς ἐφεξῆς δὲ φησὶ μέχρι τοῦ ΛΖ ἐτοὺς συμπεφω-  
νηκεναι τῇ πρὸς τὸ Δ ἐπουσία. \*

II. Μετὰ δὲ ΙΑ ἐτη, τῷ Μ καὶ τρίτῳ εἴτει, τοῦ Μεχέρ τῇ  
ΚΘ, μετὰ τὸ μεσονυκτίον τὸ εἰς τὴν Λ γενεσθαι, φησὶ, τὴν  
ἐαρινὴν ἰσημερίαν, ὅπερ καὶ ἀκολουθοῦν τῇ ἐν τῷ ΛΒ εἴτει τη-  
ρησεί.

III. Καὶ συμφωνεῖ, φησὶ, παλιν καὶ πρὸς τὰς ἐν τοῖς ἐχο-  
μενοῖς ἐτέσι τηρησεῖς μέχρι τοῦ Ν εἵλους\* ἐγένετο γὰρ τοῦ Φα-  
μενώδ τῇ πρώτῃ περὶ δυσὶν ἡλίου, μέλα μίαν ἡμέραν καὶ 5 καὶ δ  
ἐγγίστα τῆς ἐν τῷ ΜΓ εἴλει, ὅπερ καὶ ἐπιβάλλει τοῖς μέλαξιν  
Ζ εἴεσιν.

[Here intervene a page and half of Ptolemy's  
Greek text in a folio leaf]

I. Ἐνεκεν μὲν οὖν παλαιότητος, αἵτε ὑπὸ τῶν περὶ Μετώνα  
καὶ Εὐδήμονα τέτηρημεναι Θερναὶ τροπαὶ καὶ αἱ μέλα τοῦλους  
ὑπὸ τῶν περὶ Αἰσίδαρχου, οφείλοιν ἐν εἰς τὴν συγκρίσιν τῶν  
καθ' ἡμᾶς γεγνημένων παραλαμβάνεσθαι\* ἐνεκεν δὲ τοῦ κα-  
δοῦλου τε τὰς τῶν τροπῶν τηρησεῖς δυσδιακρίτους εἶναι, καὶ πρὸς  
τοῖς τὰς ὑπ' ἐκείνων παραδεδομένας ΟΛΟΣΧΕΡΕΣΤΕΡΟΝ  
εἰλημμένας (ὥς καὶ τῷ Ἰππαρχῷ δοκεῖ φαίνεσθαι) ταύτας μὲν  
παρητησαμένθ'· συγκεχρημέθ'· ἀπὸς τὴν πρόκειμενην συγκρί-  
σιν ταῖς τῶν ἰσημερίων τηρησεῖς καὶ τοῦτων ἀκριβείας ἐνεκεν,  
ταῖς ὑπὸ τοῦ Ἰππαρχοῦ μαλίστ' ἐπισημανθείσαις, ὥς ΑΣ-  
ΦΑΛΕΣΤΑΤΑ εἰλημμένας ὑπ' αὐτοῦ, καὶ ταῖς ὑφ' ἡμῶν αὐτῶν,  
διὰ τῶν εἰς τὰ τοιαῦτα κατὰ τὴν ἀρχὴν τῆς συντάξεως ὑποδείγ-  
μενων ὀργάνων ΑΔΙΣΤΑΚΤΩΣ μαλίστ' ἀτέτηρημέναις. Εξ  
ὧν εὐρίσκομεν ἐν τοῖς Τ ἐγγίστα εἴεσι μίαν ἡμέραν πρότερον  
γινομένης τὰς τροπὰς καὶ ἰσημερίας τῆς κατὰ τὸ Δ ἐπὶ ταῖς τῆς  
ἡμερᾶς ἐπουσίας.

2. Ἐν μὲν γὰρ τῷ ΛΒ εἴλει τῆς Γ κατὰ καλιππον περιόδου ἐπι-  
σημναλὸ μαλίστ' ἀπὸ τῆς μέσοπυρρῆς ἰσημερίας ὁ Ἰππαρχος, ὥς  
ἀκριβεστάτῃ τέτηρημένην, καὶ ἐπιλελογισθαι φησὶν, αὐτὴν γεγο-  
νέναι τῇ Γ τῶν ἐπαγομένων τοῦ μεσονυκτίου, τοῦ εἰς τὴν Δ φε-

\* For ΛΖ the edition of 1538 has ΔΖ, this mis-  
take happens to be immaterial in the present case; but  
proves how easily a Δ may be altered into a Λ or con-  
trary wise; it may be a mere error of the press.



ροῖος· και εἰσι το εἶος ROH απο της Αλεξάνδρου τελευτής.

3. Μείλα δε ΣΠΕ εἴη τῷ Γ εἴει Ανώνινου, ὁ εἰσι ΥΞΓ απο της Αλεξάνδρου τελευτής ἡμεῖς εἰρησάμεν ασφαλεσιὰ παλιν την μέλοπωρινην ἰσημεριαν γεγεννημενην τη Θ του Αδур, μέλα μιαν ὥραν ἐγγισία της του ἡλίου ανατολῆς.

4. Γ.πῆλαβεν ΑΡΑ ἡ αποκαλιασίας ἐφ' ὅλοις αἰγυπ-  
λιακοῖς ΣΠΕ εἴεσι (τούτέστι τοῖς ἀνατῆς) ἡμερας τας πασας Ο  
και Δ και Κ ἐγγισία μιας ἡμερας, ἀνὶ των, κατὰ την του Δ  
ἐπουσιαν, ἐπιβαλλουσων τοῖς προκειμενοῖς εἴεσιν ἡμερων ΟΑ και  
Δ. ὥστε πρότερον γεγονεν ἡ αποκαλιασίας της παρα το Δ ἐπο-  
υσίας ἡμερα Α λειπουση το Κ μέρος ἐγγισία.

5. Ωσαύτως δε παλιν ὁ μεν Ἰππαρχος φησι την, ἐν τῷ  
προκειμενῷ ΑΒ εἴει της Γ κατὰ καλιππον περιόδου, ἐαρινην  
ἰσημεριαν ἀκριβεσιὰ τήρηθεισαν γεγενεσθαι τη ΚΖ του Μεχίρ  
πρωίας· και εἰσι το εἶος το ROH απο τῆς Αλεξάνδρου τελευτής.

6. Ἡμεῖς δε την, μέλα τα ΣΠΕ εἴη τῷ ΥΞΓ απο της Αλεξ-  
άνδρου τελευτής, ἐαρινην ἰσημεριαν εὐρισκομεν γεγεννημενην τη Ζ  
του Παχων μέλα μιαν ὥραν ἐγγισία της μεσημβρίας, ὥς και ταυ-  
την την περίδου ἐπειληφεναι τας ἰσας ἡμερας Ο και Δ και Κ  
ἐγγισία, ἀνὶ των, πρὸς το Δ ἐπιβαλλουσων τοῖς ΣΠΕ εἴεσιν,  
ἡμερων ΟΑ και Δ· πρότερον ἀρα και ἐνλαυδα γεγονεν ἡ της ἐαρι-  
νης ἰσημερίας ἀποκαλιασίας της παρα το Δ ἐπουσίας, ἡμερα  
μια λειπουση το Κ μέρος. Ὡστε (ἐπει τον αὐλον ἐχει λογον  
ταύτε Τ πρὸς τοὺς ΣΠΕ και ἡ Α ἡμερα πρὸς την Α λειπούσαν το  
Κ μέρος) συναγείαι διότι, και ἐν τοῖς Τ εἴεσιν ἐγγισία πρότερον  
εἰσι, της κατὰ το Δ ἐπουσίας, ἡ πρὸς τα ἰσημερινα σημεῖα  
γινομενη του ἡλίου ἀποκαλιασίας ἡμερα Α.

7. Κ'αν πρὸς την ὑπο των περὶ Μētωνα τε και Εὐκλήμονα τε-  
τήρημενην θερῖνην τροπην, ὥς ὁλοσχερεστέρον ἀναγεγραμμενην,  
τήν συγκρίσιν παλαιότητος ἐνεκεν ποιησωμεθα της ὑφ' ἡμῶν, ὥς  
ἐνὶ μαλίστα ἀδιστάτως, ἐπιτελογισμενης, το αὐτο τουτο εὐρησο-  
μεν. Ἐκεῖνη μεν γὰρ ἀναγραφείαι γεγεννημενη ἐπὶ Αψευδους  
ἀρχοῦτος Ἀθηνησι καὶ Αἰγυπτίους Φαμενωθ ΚΑ πρωίας,  
Ἡμεῖς δε την, ἐν τῷ προκειμενῷ ΥΞΓ εἴει ἀπο της Αλεξάνδρου  
τελευτής, ἀσφαλως ἐπελογισαμεθα γεγενεσθαι τη ΙΑ του Μεσορη  
μέλα Β ὥρας ἐγγυς του εἰς την ΙΒ μεσονυχίου· και εἰσι τα μεν  
ἀπο της ἐπὶ του Αψευδους ἀναγεγραμμενης θερῖνης τροπῆς,  
μεχρι της ὑπο των περὶ Ἀριστάρχον τείρημενης τῷ Ν εἴει της  
πρωίας κατὰ καλιππον περίδου (καθως και ὁ Ἰππαρχος φησιν)  
εἴη RNB· τα δε ἀπο του προκειμενου Ν εἶους (ὁ νη κατὰ τὸ  
ΜΔ

ΜΔ εἶος ἀπο τῆς Ἀλεξανδρου τελευτῆς) μεχρι τοῦ ΤΞΓ εἶους τοῦ καλὰ τὴν ἡμέτεραν τήρησιν, εἴη ΤΙΘ. Ἐν τοῖς μεταξὺ ΑΡΑ τῆς ὈΔΗΣ διαστάσεως ΦΟΑ εἴεσιν (εἰαν ἡ ὑπο τῶν περὶ Εὐκ- τημονα τέληρημένη θερινὴ τροπὴ περὶ τὴν ἀρχὴν τῆς τοῦ Φαμε- νωθ ΚΑ ἢ γεγνημένη) προσγεγονασιν ἐφ' ὅλοις Αἰγυπτιακοῖς εἴεσιν ἡμεραι RM, ε, γ, ἐγγίστα, ἀντι RMB, ε, δ, τῶν τοῖς ΦΟΑ εἴεσι κατὰ τὴν τοῦ Δ ἐπουσιαν ἐπιβαλλουσων· ὥστε πρότερον γέγονεν ἡ ἐκκείμενη ἀποκατάστασις, τῆς καλὰ το Δ ἐπουσίας, ἡμεραις δυσι λειπουσαις τῷ IB μίας ἡμεραις. Φα- νερον ἀρα καὶ οὕτως γέγονεν, ὅτι ἐν ΟΔΟΙΣ τοῖς Χ εἴεσι τὰς δυο πληρεῖς ἐγγίστα ἡμέρας ὁ ἐνιαυσίος χρόνος προλαμβάνει τῆς καλὰ το Δ ἐπουσίας.

8. Καὶ δι' ἄλλων δὲ πλείονων τήρησεων ἡμεῖς τε το αὐτὸ οὕτως συμβαῖνον εὐρίσκομεν, καὶ τὸν Ἰππάρχον ὁρῶμεν πλεο- νακὶς αὐτῷ συγκατατιθέμενον. Ἐν τε γὰρ τῷ περὶ ἐνιαυσίου μεγέθους, συγκρίνας τὴν ὑπὸ Ἀριστάρχου τέληρημένην θερινὴν τρο- πὴν τῷ N εἴει ΛΗΓΟΝΤΙ τῆς πρώτης καλὰ καλιππον περιο- δου, τῇ ὑφ' αὐτοῦ παλιν ἀκριβῶς εἰλημμένη τῷ ΜΓ εἴει ΛΗ- ΓΟΝΤΙ τῆς τρίτης καλὰ καλιππον περιοδου, φησιν οὕτως “ Διὸν τοίνυν ὅτι ἐν τοῖς RME εἴεσι ταχίον γέγονεν ἡ τροπὴ, τῆς καλὰ το Δ ἐπουσίας, τῷ ἡμῖσι τοῦ συμαμφοτέρου ἐξ ἡμέρας καὶ νυκτός χρόνου—τὸν δὲ ἐνιαυτὸν εἴη καὶ τοῦ Δ ἐλασσον τριακοστιστῷ ἐπιλαμβανοντῇ μαλίστα μερεὶ μίας ἡμέρας.”  
Lib. 3. c. 2.

## R E M A R K S.

Παραβιβῆσθαι πρῶτον—μέλα ταῦτα ἐκβιβῆσθαι. *Para- graph 1.*] These words prove, that Ptolemy gives this account of the *autumnal* equinoxes observed by Hipparchus before that of his *ver- nal* ones, because Hipparchus himself had treated of them in the same order of *narration*: and Hipparchus was probably led to this order, be- cause thereby he followed the order of *time* in which the observations were made, his autumnal ones being *all* more antient than his vernal ones, except in the single case of the 4th in the 32d calippic year; in this year alone he made obser- vations both on the autumnal and vernal equi-  
noxes.

noxes. Hence it is plain, that since this 4<sup>th</sup> autumnal observation is included in its right order of time among the other autumnal ones, no conclusion can be drawn from the circumstance of its being thus *quoted* by Ptolemy *before* the vernal equinox in the same 32d calippic year, that it therefore *preceeded* the vernal in the order of time; it being impossible in *this single case* to arrange them *both* according to the order of time, unless the vernal one of the 32d year had been intermixed among the autumnal ones, and not each class kept separate. Accordingly this method of intermixing the vernal and autumnal ones agreeably to their *supposed* order of succession has been adopted by M. de la Lande.

[Ὡς ἀκριβοῦτατα τετηρημένων] Albategnius, Petavius and others have erroneously asserted, that Hipparchus or Ptolemy considered some of these observations as being more accurately made than others; but the above words *accurately*, &c. (ἀκριβοῦτατα, &c.) refer in the plural to *all* his observations on the equinoxes without distinction: and they are only opposed (as appears evidently at the beginning of the second passage p. 179) to the more antient observations of Meto and Euctemon, which, as he says there, were made *inaccurately* (ὀλοσχεροῦς) neither Hipparchus nor Ptolemy then give a preference to *any one* of his observations above the rest, not even to those in the 32d year. See notes above to p. 139 and 144, also the subjoined words of Petavius\*. M. de la Lande has copied this error p. 14 and 17.

\* In his observationibus quædam sunt minus accuratæ, quod & ipse Hipparchus fassus est, aliæ porro exactiores." *Doctr. temp. v. 1. lib. 4. c. 26.*



ΕΥ ΤΩ ΙΖ ΕΤΕΙ] In this 1st observation in the 17th calippic year the equinox was found, by Ptolemy's instrument, to fall at *sun-set* on the 30th and last day of the Egyptian month Mefori, the 12th and last of their year; this was coincident with the 27th of September in the 162d year before Christ, as all writers agree. But if the calippic years began at the new moons *after* the autumnal equinoxes, there could be no autumnal equinox *in* the 17th calippic year until that near its *end* in the 161st Julian year before Christ; it must therefore have been in this 161st year, that Hipparchus found the equinox to fall at sun-set on the 30th of Mefori, and not in the 162d: this 161st Julian year would be an intercalary one, and as the bissextile was inserted in February before the autumnal equinox, therefore the 30th of Mefori would in this year coincide with the 26th of September, not the 27th as in the preceeding year 162, in which all astronomers have erroneously *supposed* that this 1st observation was made; because they had erroneously *supposed* also, without the least proof, that the calippic years began at the summer solstice. Now in that preceeding 162d year Calvisius computes that the *true* equinox fell about 7 o'clock in the morning according to his own tables, that is, 11 hours *before* the observation of Hipparchus at *sun-set* \*; but if this observation was not really

\* See the words of Calvisius in note (\*) to p. 155 above; where observe, that the year 160 of Calvisius is the 162d before the vulgar æra; because Calvisius reckons backward from the supposed true year of the birth of Christ 2 years *before* the vulgar æra.

made until the subsequent year 161, the *true* equinox would in this later year fall 6 hours *later* than in 162, viz. about 1 o'clock, and this would be *only* 5 hours, not 11, before *sun-set*, where Hipparchus found it to arrive by his instrument. Still however it was in an error of being found by him 5 or 6 hours *too late*; but then the observation was made when the sun was in the horizon, therefore it may be expected to have been affected by refraction; whereby the equinox would be necessarily delayed: yet still we find that this observation was not in a *greater* error than Hipparchus himself allowed they all possibly might be, viz. of 6 hours, and many of his other observations will be found in a still *less* error; nay, none of them in fact were in an error to the amount of 6 hours, except those which were made in the horizon and were either themselves affected by the deception of refraction, or else computed by means of others so affected; whereby some of them will be found either nearly 6 hours *too soon* or *too late*. Hereby then a foundation is laid for the vindication of all the 9 observations, and of this 1st more particularly, which has been unjustly accused by M. de la Lande and others of being in much *greater* error than the rest; and indeed of so much, that he even doubts whether it be not so erroneous as to deserve to be totally rejected; for which however, as I have shewn, there would be found no good pretence, in case he had placed this observation on the right Julian year \*. The computations of the *true*

\* "Cet equinoxe est le seul des equinoxes d'automne, qu'on ne peut par aucune consideration rapprocher

equinoxes by the tables of Calvisius, or by the old Alphonfine tables may not indeed be so accurate as by the present more improved tables; but as it was by the Alphonfine or old tables, that the accusations of Petavius and others were first made, it is by the same, that they ought to be refuted. If the *true* times of the equinox, as found by the more modern improved tables, should make any difference in our reasoning and conclusions, this would be foreign to the question; as it would be an accusation arising from *ex post facto* evidence. But, I believe, by the later more accurate tables the equinoxes fall *sooner* in general than by the older ones; now this will be found to be still more in favour of Hipparchus than before, because most even of his autumnal observations, except the 1st, will be found to fall *too soon*, not *too late*, in case they were all thus made *one* Julian year later than Petavius, De la Lande and others have indeed *supposed* but not *proved*. It must be observed likewise, that the year 161 of M. de la Lande according to his *astronomic* mode of computation, as it is called, is the year 162 in the *popular* mode, just as in the case of M. Pingre's eclipses \*: he has asserted moreover, that this observation in the 17th year " is quoted

procher des autres, & il me semble, qu'il devoit être rejetté." p. 15.

\* This *astronomic* mode consists only in this, that the 1st year before Christ is considered as 0; which is attended with no advantage but detriment; for then we cannot add the years before Christ to the years after Christ without another addition of 1.

by



by Ptolemy a second time afterwards, and is then said by him to have been most accurately observed.\*" This is another mistake; it is never quoted a second time, nor yet any other *autumnal* observation, except that in the 32d year; neither is even this said to have been *accurately observed* by him in any other sense than relatively to the *inaccurate* observations of Meto, not relatively to any of *the other* observations of Hipparchus; which are in fact no where compared with each other in point of exactness, neither by Hipparchus himself nor yet by Ptolemy. It is wonderful what confusion and misrepresentation is thus every day occasioned when writers give way to their own hasty suppositions, and do not attend strictly to the words of the author whom they quote. In fine, it ought rather to have been expected, that this 1st and all the other 5 observations would have been in a much greater error than they really are, when it is considered that the effect of refraction at sun-rise and sun-set was able to produce an error of being *too soon* or *too late* in the apparent time of the two equinoxes in question to the amount of 16 hours, as M. de la Lande himself informs us.†

Μετὰ Γ' ἐτη ἐν τῷ Κ' ἐτεί] In this 2d observation 3 years after the former, Hipparchus found the apparent equinox to fall at *sun-rise* on the 1st of

\* Cet equinoxe (en 17 ann.) est cité une seconde fois dans le même chapitre, comme étant au nombre des observations les plus certaines d'Hipparque, ὡς ἀσφαλὲς ὅσατα εἰλημμεναις ἀπ' αὐτοῦ. p. 14.

† " La refraction en déclinaison sous la latitude d'Alexandrie ferait 16 heures de retard." p. 14 and 16.

the epagomenæ, therefore 12 hours *later* than before in the Egyptian retrograde year; which observation, if the calippic year did not begin until the autumnal equinox, would coincide with the morning of Sept. 27 in the Julian year 158 before Christ, not in 159 as Calvisius and others fixed it. Now on this observation Hipparchus remarks "that this equinox ought not to have arrived until *mid-day*, so that it *disagreed* he says [διωπεφωνηκεναι] with the 1st observation a 4th part of a day," *i. e.* 6 hours. But why did Hipparchus thus say, that it *ought* to have fallen 6 hours later? He has not given the reason here, but he has soon afterwards, and it is indeed sufficiently evident, namely, because the equinox must in a year like the Egyptian (which was not intercalated with 1 day in 4 years) fall 6 hours later in every year; therefore in this 2d observation, made 3 years after the 1st, it ought to have fallen 18 hours later than before: since then he found this 2d equinox apparently to fall only 12 hours later than the 1st, he perceived that there must be an error in *one* or *other* of the two observations to the amount of 6 hours; for the interval between them ought to have been 6 hours *more* than he found it apparently to be. That this was his meaning appears more plainly from these words afterwards, where he says "that on the contrary his vernal observations *did agree* with each other in respect to their falling 6 hours later every year.\*" Hence then it became evident to Hipparchus, that as his 2d observation thus fixed the *apparent* equinox at

\* Συμπεφωνηκεναι τη προς το Δεπουσια. 1 *vern. equinox.*

the distance of only 12 hours after the 1st instead of 18 hours, there must have been an error in *one* or *other* of the two observations; so that either the 1st must have been 6 hours *too late* for the *true* equinox, or else the 2d must have been 6 hours *too soon*: but which of these two was the real truth, it still remained a matter of doubt for Hipparchus to determine, neither was he possessed of any adequate means to decide this question; for he had no tables, on which he could depend, to discover to him the *true* time of these equinoxes, and it was for this very reason, that he made his own observations, in order thus to obtain more certain knowledge of what was unknown to him. However, we find by his own words, that he did at least form a *conclusion* in his own mind, that the error was in the 2d observation, not in the 1st; and that it was the 2d which was 6 hours *too soon* for the *true* equinox; for he adds, “that it *ought* to have fallen at mid-day, not at sun-rise” [δεον μεσημερινης]. Now on what *evidence* Hipparchus founded this *conclusion*, either he himself had not communicated in his own tract, or else Ptolemy has omitted to quote it as being foreign to his own subject; but we shall *afterwards* be able to point out what was *probably* the evidence which induced Hipparchus to conclude that this error of 6 hours was not in his 1st but in his 2d observation; in which conclusion however he was mistaken, as all the modern computations prove: these indeed make the error in the 1st to amount to even 12 hours *too late*; but one half of this I have already proved to be an error of the moderns themselves, so that there remain only those other 6 hours of  
error



error to account for; which last error accordingly Hipparchus himself has expressly pointed out to us and allowed. It is then indeed true that he was mistaken in thus *concluding*, that the error was in his *2d* observation, not in his *first*; but this was an error in his *reasoning* not in his *2d* *observation*; altho' indeed it was an error into which he might at first be led by the *deception* caused by refraction in his *observations* of the *vernal* equinoxes as well as of some of the *autumnal* ones: it was therefore an excuseable error, because Hipparchus had not discovered that deception, and it did not arise from negligence in himself. But the moderns having discovered the deception and being thereby enabled to ascertain, that the error of 6 hours in question subsisted in the *1st* observation, not in the *2d*, they ought at the same time to have candidly justified Hipparchus from any greater errors than *first* of all one of 6 hours *sometimes* in *observation*, of which he was himself sensible, though ignorant of the cause, namely, a deception by refraction; and *secondly*, another error in *reasoning*, derived from the former, by his concluding the above error of 6 hours to subsist in the *2d* observation not in the *1st*; but it is however only in the *first two* of his observations that this latter error in *reasoning* occurs, for in his *conclusions* concerning all his later 4 observations we shall find that he was right.

Thus then, when the moderns shall have corrected *their own error* in placing *all* the six autumnal observations one year *too soon*; and again shall have corrected *their own neglect* in not making allowance *on the right side* for that error of 6 hours, which Hipparchus confessed to subsist

in *one* or *other* of the first two; but which he indeed erroneously *concluded* to be in his 2*d* by its being 6 hours *too soon*, although the error really was in his 1*st*, by the latter being 6 hours *too late*: when these two modern errors shall be corrected, it will be found that the pretended errors of Hipparchus in his observations never will exceed those 6 hours which he himself has allowed and pointed out to us; and which are not always the *same way*, but err sometimes by being 6 hours *too soon*, and at other times by being 6 hours *too late*, yet sometimes however he is quite *right* or nearly; as will appear more clearly by the two following Statements, first of the hours of error, which Calvisius imputes to these observations, and secondly of the hours of error, which really subsist in them, when the above *two corrections* are made.

FIRST

FIRST STATEMENT.

*The supposed Hours of Error in the autumnal Observations of Hipparchus, according to the Dates of Years and Computations of the true Equinoxes by Calvisius and other Astronomers.*

	Bef. Chr.	Calip. Years	Sept. 27	According to Calvisius	Too late
1	162	17	at Sun-set	11 Hours	{ De la Lande says 12 Hours
2	159	20	at Sun-rise	6 Hours	{ De la Lande says 6 Hours or more, p. 15.
3	158	21	at Mid-day Sept. 26 and 27	7 Hours	{ Riccioli says 6 Hours
4	147	32	at Mid-night	1½ Hours	{ Boulliaud says 6 or 8 Hours
5	146	33	at Sun-rise Sept. 26.	1½ Hours	{ De la Lande says 6 Hours *
6	143	36	at Sun-set	2½ Hours	Too soon

\* "Cet equinoxe etoit arrivé dès minuit." p. 18.

SECOND STATEMENT.

*Hours of Error in the Observations of Hipparchus according to the real Dates of Years in Ptolemy's Text, and the Computations of the true Equinoxes by Calvisius for the preceeding Years, but adapted to these succeeding Years, by allowing them to fall 6 Hours later than in the preceeding Years.*

	Bef. Chr.	Calip. Years		According to Hipparchus	
1	161	17	Sun-set	Sept. 26	{ Too late 5 Hours only according to the Computation of Calvisius
2	158	20	Sun-rise	27	Right by the same
3	157	21	Mid-day	26	Too late 1 Hour
4	146	32	Mid-night	26 & 27	Too soon 4½ Hours
5	145	33	Sun-rise	26	Too soon 4½ Hours
6	142	36	Sun-set	26	Too soon 8¼ Hours



By this latter statement it appears that if these 6 observations were all made one Julian year *later* than hitherto supposed, the errors of Hipparchus would be always *less* than 6 hours, except in the single case of the 36th year; but as the present more correct tables place the true equinoxes in general *sooner* than those old tables made use of by Calvisius, consequently in the 3 last years above, the errors will be hereby *reduced*, not augmented, as many hours as the modern tables place the equinoxes *sooner* than the antient ones: and it must be remembered moreover, that if the errors in the three last years should, by means of more accurate computations, amount even to 6 hours *too soon*, yet Hipparchus himself has pointed out and cautioned us concerning these errors; for he expressly says of that 4th equinox in the 32d year, that it *ought* to have fallen not at midnight but at sunrise 6 hours *later* [δεον πρωιας]. The same again concerning the 6th equinox in the 36th year, he says that it *ought* to have arrived not at sun-set, but at midnight 6 hours *later* [δεον του μεσονυχτιου]. The same is in course necessarily the case with the 5th in the 33d year; for he remarks on this, “that it followed after the preceeding observation at the right interval which it *ought*, viz. that of 6 hours for its being made one year later” [οπερ και ην ακολουθον τη προ αυτης τηρησει]. But as that preceeding 4th observation in the 32d year, was found by his own private *reasoning* to be 6 hours *too soon* for the *true* equinox, consequently the subsequent one in the 33d year must be 6 hours *too soon* likewise. So that in all these three last equinoxes, which his *observations*

tions (as we see) had placed *too soon*, he has himself corrected *their times* of arrival by his *reasoning* or computation, and informed us that they all three *ought* to be found 6 hours *later* in order both to *agree with* [συμπεφωνημένοι] each other and with the *true* time of the equinox. Still however he has not even here informed us on what *evidence* he depended in his *reasoning*, that the *disagreement* between his observations in the two years 32 and 36, to the amount of 6 hours, did not arise from the observations immediately *preceeding* them being 6 hours *too late*, but from those two *subsequent* observations being 6 hours *too soon*; yet whatever his *evidence* was, it appears at least by the result, as contained in the *second statement* above, that he was right in his *reasoning* concerning the *three last* examples in 32, 33 and 36, although he was mistaken, as I have shewn, in the *first* example concerning the years 17 and 20. Thus we find here again the *harmonious effects* which follow from placing these 6 observations each 1 Julian year *later*, agreeably to the dates actually pointed out by Ptolemy's present text, and as exhibited in the *second statement*; for this single variation necessarily takes away *the chief* quantity of error imputed by astronomers to the first 3 observations, and at the same time renders the hours of his observations in the last 3 years conformable to *his own* computation of their being 6 hours *too soon*, and of their wanting to be corrected by being all placed 6 hours *later*; which *correction* will actually make the 4th, 5th and 6th observations to be nearly coincident with the *true* hours of

those equinoxes, as they have been computed by Calvisius. Thus then all is in harmony.

The following farther remarks however may be still useful to shew still farther the *harmonious effects* arising from the fact, that all the 6 autumnal observations were made 1 year later than hitherto supposed, together with the *discordant effects* arising from the contrary opinion. Now it may be observed, that the error of 6 hours in the observations of Hipparchus with respect to *each other* is never found in those observations made in two immediately successive years, but only after an interval of 3 or 11 years; also that it occurs only in the *autumnal* observations; for he expressly notices, that his *vernal* ones “*did follow each other at their right intervals of 6 hours later for every year down from the very first of them in the 32d year to the last of them in the 50th year*”\*. He had however remarked, in regard to his vernal observation in the 32d year, that his own private instrument placed that apparent equinox at sun-rise, and 5 hours *sooner* than a public instrument erected at Alexandria; which both he himself, and Ptolemy also, imputed to an error in the position of the public instrument †, whereas it *might* in reality have been owing to the effect of refraction on his own instrument at sun-rise; *in case* it be indeed certain (as the computations of Cal-

\* Συμπεφωνημέναι, φησι, τη προς το Δ επουσία—και συμφωνει μεχρι του Ν ετους. 1 and 3 vern. equinox. p. 179.

† Ο κρικος δε, φησιν, εν Αλεξανδρεια ισον εξ εκατερου μερος παρηνασδη περι ε ωραν ωστε ηδη και την αυτην ισημεριαν διαφορως τετηρημενην ε ωρας εγγιστα διενεγκειν. *ibid.*

visius,



visius, Cassini and de la Lande pretend) that Hipparchus' own instrument, in placing that equinox at sun-rise, placed it 5 or 6 hours *too soon*\* : but at least Hipparchus has made no mention of his having discovered *any such error* in his vernal observations, and we shall afterwards find some reason *to doubt* whether they really were in an error of being 6 hours *too soon*, as the following *statement* of them, according to the computations of Calvisius and others does indeed affirm. But in fact Hipparchus was himself possessed of no such adequate means to discover when his observations *disagreed* with the *true equinoxes*; although he could perceive by the interval between his observations when they *disagreed* with *each other* in regard to their following or not at the proper interval after each other : he did nevertheless attempt to determine the *true times* of those equinoxes by *computation*, as I have shewn in regard to his autumnal observations, and attempted it also with some success in all those observations, except the first, in case the *second statement* above be the right one; and it still remains a matter of *some doubt*, whether his vernal equinoxes likewise do not in fact agree more with the *true equinoxes*, than has been supposed hitherto, the foundation for which doubts will appear in p. 199 and note p. 204;

\* " Je supposerai comme M. Cassini, que l'équinoxe arriva le 23 Mars 145, à 23h. 55'." p. 18. This means at 55' after 11 of the morning of March 24 in 146 according to the popular reckoning; and thus it nearly agrees with the public instrument at Alexandria, and with Calvisius. See p. 196.

But I myself undertake nothing more than to point out matters of *fact*, and leave all *computations* relative to them for the consideration of others.

### A STATEMENT.

*Of the Hours of Error in the Observations on the vernal Equinoxes by Hipparchus according to the Computations of their true Times by Calvisius and others.*

	Bef. Chr.	Calip. Year	March 24	Too soon according to Calvisius
1	146	32	at Sun-rise	{ 4 Hours *: but Caffini and de la Lande say near 6 Hours.
2	135	43	{ at Mid-night between 23 & 24 }	{ 2 Hours; but de la Lande says 6 hours. p. 19†
3	128	50	at Sun-set Mar. 23	{ 1 Hour: but others say 1h. $\frac{3}{4}$

\* See the foregoing note in regard to the computation of Caffini and De la Lande. But with respect to Calvisius, his words are “*hæc observatio accidit die 24 Martii; nostræ tabulæ monstrant æquinoctium idem horis decem post meridiem*” (ant. Chr. 144). This means at 10 o'clock at night on the 24th: I suspect therefore that *meridiem* must be an error of the press for *mediam noctem*, i. e. 10 o'clock in the morning of March 24, and 2 hours *sooner* than de la Lande computes; Unless Calvisius has omitted *Martii 23*, and thus meant 10 at night of March 23, for which date we shall afterwards see some evidence arising from a computation of de la Lande.

† “Il faut que l'équinoxe soit arrivé réellement le 24 au matin.” p. 19.

On this equinox M. de la Lande remarks “*Ptolémée dit, que cet equinoxe s'accorde avec le 5me* (which is my

By this statement of the vernal observations, if compared with the *second* statement of the autumnal ones, it does not appear that the vernal have any such claim to superior accuracy as Petavius and others have ascribed to them\*; I do not mean however with respect to the *real truth* of their *times*, but only with respect to *such representation* of the times as Calvisius, Cassini, Boulliaud and de la Lande have made of them; for I reason only on the foundation of their computations and assertions: I point out however these *computations* as matters, which still want to be more accurately ascertained and reconciled both with one another, with themselves and with truth; whereby they will be possibly still more reconciled with the *observations* of Hipparchus;

my 1st vernal) il n'y a en effet que 3h.  $\frac{1}{2}$  de *difference*." Now *s'accorde* and *difference* are the phrases by which M. de la Lande translates *ἢν ἀκολουθον* and *διαπεφωνηκεται*: and according to *Ptolemy's* meaning of those phrases there was in reality *no disagreement* whatever between those two observations; for the 2d followed exactly at 11 times 6 hours after the 1st, at the end of 11 retrograde years. Since then M. de la Lande finds a *disagreement*, he must mean a disagreement in some different respect from what *Ptolemy* meant: M. de la Lande may be right according to *his own* meaning, but the same was not the meaning of *Ptolemy*, and yet he objects to what *Ptolemy* says; he must ascribe therefore to *Ptolemy* a different sense from his real one, otherwise M. de la Lande could not have reason to *correct* him; for *Ptolemy* was *strictly right* according to his own sense of *s'accorde* and *difference*, or rather I should say Hipparchus.

\* See above, p. 139, in note.

but



but let the *distant* and unexplored consequences be what they may, I confine myself to that reconciliation, which results as the first and immediate consequence from the computations *already presented* to us by those astronomers. Now this first consequence is, that all these 3 vernal observations did not at worst exceed the 6 hours of error, to which Hipparchus confessed that all his observations might be subject, and that they all three placed the apparent equinoxes *too soon*: this was indeed the very error, into which it was most natural for the vernal observations of Hipparchus to fall; because they were chiefly made at sun-rise and sun-set, therefore would be affected by refraction, which in the vernal equinoxes would cause them to appear *too soon*. So far then all is *in harmony* both with *probability* and with the express words of Hipparchus: it must however be still reserved as a matter of *doubt*, which wants to be better ascertained, whether these three *vernal* ones did fall so much *too soon*, as all these astronomers agree in.

But this *harmony* will be found moreover to extend itself from these *vernal* to the *autumnal* observations. For hereby we may perhaps be able likewise to discover what the *evidence* was on which Hipparchus depended, when, as mentioned above, he affirmed, that all his last three *autumnal* observations fell *too soon* by 6 hours. His *evidence* might probably be this, the autumnal equinox in the 32d year fell in the night, when its hour of arrival could not be *observed*; he could only observe at sun-set *before* its arrival, and at the following sun-rise *after* its arrival, and fix the hour between the two as well as he could.

Now

Now that he might do this the more accurately, he probably would call in to his assistance a *computation* from his preceeding *vernal* equinox of 32, in order to determine at what hour that computation would end; and this would be *at sun-rise* exactly where he says, that the equinox *ought* to be found, viz. 6 hours *later* than where his observation had placed it in the night. For Hipparchus had doubtless adopted the same interval as Ptolemy of 187 days between the vernal and autumnal equinox, when the former preceeds, which both Boulliaud and de la Lande approve\*. But from *sun-rise* on Mechir 27, where Hipparchus found the vernal equinox in the 32d year, down to *sun-rise* on the 4th of the epagomenæ, where he says the autumnal equinox *ought* to have fallen (δεον πρωιας) are exactly 187 days†. By this *computation* then it probably was, that he concluded the *observation* at the preceeding *mid-night* to be 6 hours *too soon*. The same method by *computation* he probably employed also in the two remaining autumnal equinoxes

\* Απο εαρινης ισημεριας μεχρι θερινης τροπης ημεραι 5Δ, 5 (i. e. 94, 12) απο θερινης τροπης μεχρι μετοπωρινης ισημεριας 5Β, 5 (92, 12) in all therefore 187 days. Lib. 3. c. 4.

† For from *sun-rise* on Mechir 27 the 6th month, to the end of that month are 4 days; and 6 months more of 30 each, make up 184 days; and 2 days 18 hours more to *mid-night* in the 3d of the epagomenæ, make in all 186 days 18 hours to the hour of the *observation*; and 6 hours more to *sun-rise*, where Hipparchus affirmed that the equinox *ought* to fall, make the 187 days required.

in the 33d and 36th years, and hence he *affirmed*, that these also fell 6 hours *too soon*. Neither is it merely conjecture, that his *evidence* for this affirmation was such a *computation* from his vernal observations; for Hipparchus expressly says, that he made use of *computation* to fix this autumnal equinox in 32. “Hipparchus says, that he had *computed*, that the equinox in the 32d year happened on the third of the epagomenæ at mid-night.\*” επιλελογισθαι means *computed*, the word constantly employed for *observed* is ετηρησε. It may be remarked, that Ptolemy also employs the same words επιλελογισμενης and επιλογισαμεθαι, when he speaks of the determination of the summer solstice by himself, which in like manner fell in the night, when it could not be *observed*; and in the determination of which he likewise probably called in to his assistance a *computation* from the vernal and autumnal equinoxes in the same year, both of which he mentions as having been *observed* by himself.† This *evidence* at the same time proves, that Hipparchus did not speak at random, when he affirmed, that the cause of the want of due interval between his last 3 autumnal equinoxes arose from those in the 32d year and in the 36th falling 6 hours *too soon*: but this could not possibly be true, if these autumnal observations had been made in the years marked in the *first statement*, and as the astronomers commonly suppose; for

\* Επιλελογισθαι, φησιν, αυτην γεγονεναι τη Γ των επαγομενων του μεσονυκτιου. See paragraph 2. p. 179.

† See paragraph 7. p. 180.



in those years the *apparent* equinoxes, as found by Hipparchus, would have all, except one, fallen actually *too late*; how then could Hipparchus affirm, that they *ought* to have fallen 6 hours *still later*? But this affirmation, agrees most happily with the last three autumnal equinoxes, in case they were made in the years marked in the *second statement*; and even in the first of these 6 observations it reduces his error within his own prescribed bounds of 6 hours\*. Here then we discover farther *harmonious effects* resulting from the years as given in the *second statement*, and these also such as are *necessary* to render the *words* of Hipparchus consistent with the real truth of the *facts* as well as with the *real dates* in Ptolemy's text†; why then do astronomers alter the *date* 178, which alteration necessarily produces *discord*?

But here however, it must be remarked, that one circumstance presents itself, the only one of which I have any knowledge, and which may

\* Οὐκ ἀπελπιζω καὶ ἡμᾶς καὶ τὸν Ἀρχιμήδην καὶ ἐν τῇ τήρησει καὶ ἐν τῷ συλλογισμῷ διαμαρτανεῖν καὶ ἕως τεταρτοῦ μεροῦς ἡμέρας. Here we see again, that he says he employed *computation* (συλλογισμῷ) as well as *observation* (τήρησει) to assist in fixing some of his equinoxes.

† Hence also we may possibly discover the *reason* (as hinted above) why Hipparchus conceived his 2d autumnal observation to be 6 hours *too soon*, when the real truth was, that his 1st was 6 hours *too late*; for finding that his 4th, 5th and 6th were 6 hours *too soon*, he hastily concluded that the error in the interval between the 1st and 2d was of the same kind, by the 2d being *too soon* like the rest, though in fact it was not, but quite *right*.  
be

be thought to offer an *objection* to the autumnal observations having been made in those years marked in the *second statement*. This objection is, that it is the natural effect of refraction at the autumnal equinoxes to cause them to appear *too late*, whereas in the second statement the last 3 appeared *too soon*, and this notwithstanding that 2 of them were made when the sun was in the horizon, at which time refraction has most effect; this is contradictory, it may be said, to *natural probability*. But in opposition to this it must be remarked likewise, that nevertheless the first 3 out of the 6 still fell *too late*, if they erred at all; and that the 3d did so in the *first statement*, although made at mid-day, when there is no refraction; moreover, that the 6th fell *too soon* likewise even in the *first statement* as well as in the *second*; consequently we cannot get free of all offences against *natural probability*, not even in the *1st statement* any more than in the *second*: if then Hipparchus could in his observations fall into such errors (supposing the first statement to be the right one) as should in that offend against *natural probability* in *one respect* and in *one degree*, why might he not be equally capable (in case the second statement be the right one) to fall into errors, which should in this also offend against *probability* in *some other respect* and *some other degree*? more especially, since he himself informs us by his own *words*, that his errors and offences were actually in *this other respect* last mentioned, and even *precisely* in *this other degree* likewise, namely, that his observations placed the three last equinoxes 6 hours *too soon*, which  
is

is the very error in *kind* and nearly in *degree* found subsisting in them according to the second statement: but notwithstanding this error, they are still less erroneous, upon the whole, in the second statement than in the first; consequently, upon the whole, they are more consistent with *probability* in the second than in the first statement. This second statement then, by reducing the errors of Hipparchus to be consistent with what *his own words* attribute to them both in *kind* and *degree*, does in fact absolve him from the imputation of error at all; for errors discovered by the author himself, made known to us and ascertained by him, cease in reality from being any longer errors; because they actually in that case point out the truth, when *thus corrected* by himself. Hereby then Hipparchus becomes sufficiently vindicated from the accusations of modern astronomers\*.

\* I may add here however as a farther vindication, that in reality the circumstance of the *autumnal equinoxes* in the 32d, 33d and 36th year, falling about 4 hours *too soon* in the second statement, is not altogether such an offence against *natural probability*, as it may at first sight appear to be: for there were *two causes* which might produce error in observations by the *Armilla*, and these tend to produce errors, which are diametrically opposite to each other. One is, as above-mentioned, *refraction*, which in the autumnal equinoxes naturally tends to make them appear *too late*; but a second cause of error, is from the *parallax* of the sun, and this tends to make them appear *too soon*, as is thus pointed out by Riccioli, “ Duo vitiant observationes, nisi corrigantur, primum est *parallaxis solis*; nam æquinoctia autumnalia *præcipitat* & verna *retardat*, exhibendo solem nobis depressiorem, quam si ex centro terræ



But will it be as easy to vindicate M. de la Lande from error and discord in his *computations*, as Hipparchus in his *observations*? Let others examine the following example. I have shewn in my note to p. 195, that M. de la Lande agrees with Cassini in regard to the *vernal* equinox of the 32d calippic year, falling about *mid-day*, that is, 6 hours *later* than where Hipparchus had fixed it by observation; which was at *sun-rise* on March 24, in the year before Christ 145, according to the astronomic reckoning of M. de la Lande, which is the year 146 in the popular reckoning: He then says a few lines lower, "That in the *same year* Hipparchus observed the *autumnal* equinox at *sun-rise* on Sept. 27 (which was according to M. de la Lande, the autumnal equinox in the 33d calippic year) but by its being affected by refraction, it appeared *too late*; and it had really arrived 6 hours *sooner*, viz. as early as the preceeding *mid-night*.\*" Now be-

terræ spectaretur: alterum & majus vitium est a *refractione*, quæ attollendo solem verna æquinoctia *precipitat*, autumnalia *retardat*." *Almagestum nov. Bonon.* 1651. *Tom. 1. pars prior. p. 133.*

\* La même année (145) l'équinoxe d'automne fut observé le 4 jour intercalaire au *matin*, ce qui revient au 26 Sept 145 à 16h. 1'.—mais à raison de la refraction il parut trop tard, il étoit arrivé dès *minuit*." p. 18. This means *mid-night* between the 26th and 27th of Sept. in 146 according to the popular mode, and thus on reckoning back 187 days, it would place the vernal equinox in 146 about 6 hours *sooner* than Hipparchus, not *later*, and only 2 hours sooner than Calvisius, in case we suppose his word *meridiem* to be the right reading,

tween *mid-day* on March 24, and *mid-night* between the 26th and 27th of September are included only 186 days and 12 hours, whereas there ought to be an interval of 187 days between the vernal and subsequent autumnal equinox. Here is an error then of 12 hours; how can *both* these computations of M. de la Lande be true? It is very possible therefore, that *neither* of them may be true. But if it be said, that M. de la Lande's phrase, *I will suppose* (*Je supposerai*) does not express his *full assent*, but that he admitted what Cassini said *for the present* only, still the consequence must be, that Hipparchus's *vernal* observation in 32 was found by him 6 hours *too late* not *too soon*; and if so, then this again is an offence against *natural probability*, and if such an offence does subsist in this vernal equinox, why may it not equally subsist in regard to the 3 last autumnal equinoxes in the second statement? It was for these reasons then that I expressed some *doubt* above, whether the vernal equinoxes of Hipparchus were really found by him *too soon*: but at least here is a *discord* which wants to be removed; and until it be removed, it proves that the observations of Hipparchus have been *sometimes* accused by *some* authors upon precarious principles of modern *computation*, as well as upon precarious *corrections* of Ptolemy's Greek text. If then such doubts, difficulties and discord are

ing, and that *Martii* 23 is omitted. See my note (\*) to p. 196, which refers to the present note. But at this rate this vernal observation by Hipparchus was *too late* not *too soon* for the true equinox.

P

even

even still found subsisting in modern computations, where is there any solid ground for accusing the integrity of Ptolemy's text, or the accuracy of the *second statement* above of the observations and errors of Hipparchus; which is founded on Ptolemy's *present text*, and for the alteration of which we have not hitherto been able to discover any sufficient evidence?

It is moreover remarkable, that this very objection of a want of *due interval* between 2 equinoxes, which thus lies either against the consistency of the above two computations of M. de la Lande *with each other*, or else against the consistency of these two computations by M. de la Lande *with those* of Cassini and other astronomers, is the *same kind* of objection, which M. de la Lande himself *approves*, when urged by Bouillaud against the integrity of the date 178 as mentioned in my note to p. 134; or (what comes to the very same thing) against the *precedence* of the vernal observation of 32 to the autumnal observation of 32: for Bouillaud in regard to this urges, "that the above vernal could not *precede* the above-mentioned autumnal equinox, because there would then have been found by Hipparchus only 186 days and 18 hours between those two equinoxes; which would want 6 hours of the *due interval* which ought to subsist between them."\* This is only *one half* of the error contained between M. de la Lande's

\* "Si vernale istud (anno 32) prius esse autumnali (anno 32) dicemus, reperiemus tantum *intervallum* a verno ad autumnale dierum 186, hor. 18." *Astron. Philol.* p. 64.



two computations, and yet he approves this objection by Bouillaud, which applies with double force either against himself or else against those computations, on the supposed accuracy of which others have accused Hipparchus of error and Ptolemy's text of corruption. But this objection, although it is indeed valid against M. de la Lande or Cassini, yet has no force when urged as above by Bouillaud against the *precedence* of the vernal observation of 32 to the autumnal one of 32; because Hipparchus allows, not only *in general* that his observations were subject to error to the above amount of 6 hours, but also *in particular* he expressly notices, that the autumnal observation in 32 was in an error of being 6 hours *too soon* (δὲν πρόωγος); which 6 hours, if added, would complete the 186 days and 18 hours to the *due interval* of 187 full days. It seems wonderful then how Bouillaud could urge so weak an objection, or how M. de la Lande could approve an objection of *this kind*; the above-mentioned one is indeed Bouillaud's 3d proof; but it is on his 2d proof that M. de la Lande bestows approbation, and which is as follows, of the *same kind* however with the foregoing one. Bouillaud here contends "that the autumnal observation of 32 *must precede* the vernal of 32, because there actually was the right interval of 178 days 6 hours between them."\* But has not Hipparchus himself remarked, that his autumnal observation of 32 fell 6 hours *too soon*

\* "Ab autumnali (anno 32) ad vernale (anno 32) elapsi sunt dies 178 hor. 6, quantum *debet* esse intervalum." *Ibid.*

(*δεν πρῶτος*) which his own dates there prove? If then these 6 hours be deducted from the 178 days 6 hours, it reduces the interval to 178 days only, and thus becomes a proof, that the autumnal could not *precede* the vernal of 32; so that this is, if possible, worse than Bouillaud's 3d proof above: but in both he gives unlimited credit to those *observations* of Hipparchus, which the latter himself allows to be erroneous; yet Bouillaud will give no credit to the *corrections* of those observations by Hipparchus, which corrections the dates given there prove to be right. What is all this but cross-purposes? But at best no solid *conclusion* can be drawn hence either way, until it be first determined, whether the *observations* of Hipparchus are intitled to more credit than his own *corrections* of them: yet these are the kind of proofs which M. de la Lande approves.\* Bouillaud's remaining proof, which is the 1st in his order is nothing more solid†. Such then are our

\* “Bouillaud fait voir, qu'il faut lire *ann.* 177 (& non *ann.* 178) parceque les années calippiques commençaient au solstice d'été; il le *prouve* aussi par l'*intervalle* de 178 jours 6 heures, qu'il y a de cet equinoxe autumnale (en *ann.* 32) au suivant (l'equinoxe vernal en l'*ann.* 32).” *Memoire, &c.* p. 17.

† The defect in this is in the *premises*; for Bouillaud sets out with *supposing* and *asserting* as true what cannot be proved true, viz. that there was an interval of 285 full Egyptian years and 70 days between the two autumnal observations of Hipparchus and Ptolemy. “Si annos tropicos (prout taxavit Ptolemæus) 285, seu annos Egyptios 285 dies 70, auferamus, &c.” *Ibid.* Here we see he *supposes* what can neither be proved by the phrase *μετα σπε ετη* nor by any other in Ptolemy, as I have shewn above and shall confirm more afterwards.

philos-

philosophers, who have thus themselves in their own sciences for two whole centuries, outdone the wanderings of the Israelites in the wilderness, while they nevertheless profess to clear up all our ideas in theology! Philosophers, physical and metaphysical, first cure your own selves; until then philosophy can be deemed no better than a quack medicine in religion! yet thus daily does it impose on the public with a learned face of solemnity, while destitute in fact both of solid argument and evidence!

ΜΕΤΑ ΔΕ ΙΑ ΕΤΗ] In this sentence, and in several more before and after it, we have repeated examples of the same phrase as in the case above-mentioned of ΜΕΤΑ ΟΠΕ ΕΤΗ: here then we might hope to meet with some illustration of the true meaning of this numeric phrase by ΜΕΤΑ: yet it unfortunately happens, that these examples are of no use for that purpose; because through the *circumstances*, which accompany them, the *precise intervals* in all these cases must necessarily be either *equal* to the full number of Egyptian years there specified, or *more* than equal. What we want then to meet with are some examples, in which the *precise intervals* must necessarily on account of the concomitant *circumstances* be *less* than the number of full years annexed; as I shewed to be the real case in the *first* sentence in regard to ΜΕΤΑ ΟΠΕ ΕΤΗ, if the *date* 178 be not corrupted: such examples of a *less* interval would prove to a demonstration, that the phrases ΜΕΤΑ ΙΑ ΕΤΗ and ΜΕΤΑ ΟΠΕ ΕΤΗ and all such others were never meant to express any thing more precise than merely *in* and *upon* the last of the years there specified; for *this sense* leaves those in-



tervals to be either *equal to*, or *more* than equal, or *less* than the full number, according as the observations were made near the *beginning* or the *end* of the first and last years. But here we again meet with ill luck; for this numeric phrase by *μετα* is never employed any where else by Ptolemy himself in all his work with respect to *years*, except in that single case of *μετα σπε ετη*; which is, as the antiquaries speak, an *unique*: he does indeed sometimes make use of that phrase by *μετα* in regard to *hours*; but his reasoning in these instances relates to differences too minute and too little obvious to afford us any satisfactory illustration.\* The present sentence *μετα ια ετη* and the concomitant ones are not the words of Ptolemy, but of Hipparchus, they being all extracts quoted from him; in imitation of which Ptolemy is once, and once only, led to adopt the phrase of Hipparchus in the case of *μετα σπε ετη*, but in all other examples Ptolemy himself makes use of *μεταξυ*; and unfortunately the above extracts quoted from Hipparchus are of no use to us for our purpose in view. However, this other numeric phrase by *μεταξυ*, although not exactly a parallel one, yet is sufficiently so to illustrate the vague meaning of the phrase by *μετα*; by proving that *μεταξυ* does in like manner sometimes express an *interval*, which is *more* than the number of full years annexed, and sometimes *less* than that number of full years; therefore that *μεταξυ* means like *μετα* only *in* and *upon* the last of the years so

\* See one example with respect to *hours* at paragraph 7 at the bottom of p. 180.

specified. Let us point out some examples of these *different* intervals thus expressed by μεταξυ. In the following example the *precise interval* between the two observations in question is at least 118 days *less* than the 12 years here said by Ptolemy to *intervene between* them, let the years be reckoned in any possible manner\*. Ptolemy here says, “ that Timocharis made an observation on the moon and the star Spica, on the 5th day of the Egyptian month *Tybi*, and in the 454th year of Nabonassar: Again, that he made another observation on the same two objects on the 7th day of the Egyptian month *Thoth* and in the 466th year of Nabonassar: the interval *between* (μεταξυ) the two observations being 12 years.” Now did he mean here the *precise interval*, and that it was at least exactly equal to 12 full Egyptian years, if not *more*? Certainly neither of these could be his meaning; and he could not mean the *precise interval* at all; because if this be reckoned up, it will be found

\* Τιμοχαρις μεν αναγραφει τηρησας εν Αλεξανδρεια, διδω τῷ ΑΓ ελει της πρώτης καὶ α καλιππον περιόδου, του δε Τυβι τη Ε, ἡ σεληνη τον Σταχυν κατελαβε, &c.—και εστιν ὁ χρονος κατα το ΥΝΔ ετος απο Ναβονασαρου κατ' Αιγυπτιους Τυβι Ε—

Και εν τῷ ΜΗ δε ετει της αυτης περιόδου φησιν ὁμοιως, ὅτι του δε Θωδ τη Ζ—ὁ Σταχυς εφαινετο ἀπτομενος, &c.—και εστιν ὁ χρονος κατα το ΥΞΓ ετος απο Ναβονασαρου κατ' Αιγυπτιους Θωδ Ζ—

Και ὁ Σταχυς αρα δια ταυτης της τηρησεως νοτιωτερος μεν παλιν ην, &c.—εν τοις ΙΒ ετεσι τοις ΜΕΤΑΞΥ των δυων τηρησεων. *Lib. 7. c. 3.*

N. B. The two above calippic dates are the same with the 2d and 4th *Athenian* dates referred to in my Tables.

to want 118 days of 12 full years, and thus to be *less* than the number of the 12 full years there specified \*. He could therefore mean both here and elsewhere by *between* (μεταξύ) only the *general and gross interval*, if reckoned in a popular way by *whole* years, and these computed from the *end* of the year 454 exclusively, in which the first observation was made to the *end* of the year 466 inclusively, *in* and *upon* which the second observation was made; *between* which dates there intervened in reality 12 full Egyptian years. Thus then we find, that even this numeric phrase by μεταξύ does not mean *after* the *expiration* of the 12 full years here specified, but

\* For from the 5th of Tybi (the 5th Egyptian month) there are 25 days more to the end of that month; and 7 months more of 30 days each with the 5 epagomenæ to the end of the Egyptian year 454, amount in all to 240 days, which added to the 11 years from the end of 454 to the end of 465 make up 11 years and 240 days; to which adding the first 7 days to the 7th of the first month Thoth in 466, they amount to only 247 days more than 11 full years, therefore are *less* than 12 full years by 118 days: And *universally* this must always be the case, whenever the day of the 2d observation falls *sooner* in the Egyptian year than the day of the 1st observation; on the contrary when the 2d falls *later* than the 1st, then there will be *more*, than the annexed full number of years, in the *precise interval* reckoned from the *day* of one observation to the day of the other. So that whenever Ptolemy reckons by *whole* years he means only the *gross interval*; and when he wants to ascertain the *precise interval* he always adds the odd days to the *whole* years; of which latter we have an example at the bottom of p. 118 above.

only



only *in* and *upon* the 12th. or last of the number of years there annexed: much more may we conclude therefore that Ptolemy meant the same by *μετα* in his phrase *μετα σπε ετη* (because this is its proper and common sense in other antient authors) viz. not *after* the *expiration* of 285 years, but only *in* and *upon* the 285th year, reckoned from the *end* of the year 178 exclusively, *in* which the observation of Hipparchus was made; so that the *precise interval* became *less* than 285 full years, *i. e.* only 284 years and 70 days in the first sentence, notwithstanding that the same phrase in the second sentence (on account of that *different circumstance* pointed out in the note above) included an interval of *more* than 285 full years, *i. e.* 285 and 70 days. The above parallel passage then by *μεταξυ* may be called a case in point, and fully vindicates the integrity of the *date* 178, by shewing, that it is not *inconsistent* with the number of years contained in the *sum* 285, as Copernicus had *supposed*. Because in the first place this latter sum by *μετα* just as well as by *μεταξυ*, expresses only the *gross interval* reckoned by *whole* years from the *end* of the year of the 1st observation in 178 to the *end* of the year of the 2d observation in 463 inclusively (which inclusive mode I have often mentioned before, as being the general mode of reckoning by the antients, though not the universal one) and this *indefinite mode* of reckoning leaves the *precise interval* to become *more* or *less* than the *sum* of *whole* years, according to the following circumstance; for in the second place, the fact is that in the 1st sentence, the 2d observation fell *sooner* in the Egyptian year,

than

than the 1st observation, but on the contrary in the 2d sentence the 1st observation by Hipparchus fell *sooner in the Egyptian year* than the 2d observation by Ptolemy; it is this *different circumstance* which causes the difference in the length of the *precise interval* in the two cases; to which different circumstance, and the consequences resulting from it in the *two sentences and computations*, none of the astronomers have attended. Of these it may therefore be truly said, “that they have all spoken evil of that which they knew not;” which has indeed been long ago pointed out to us in scripture as being too often the character of philosophic scorners, who with their brethren the metaphysical Unitarians and pretended rational christians daily fall into greater errors, extravagancies and absurdities than any other class of men in literature; and these indeed of such a nature as to make one quite sick of the very name of philosophy, when thus perpetually abused to support, by disputation, the cause of negligence, prejudice, servility to authority, fancy, sophistry and learned romance in opposition to the dictates of *rational enquiry*.

In the very same page Ptolemy gives us two other examples, in which the *gross interval* between the two respective observations, is again, in like manner, reckoned by *whole years* and expressed by μεταξὺ; but in both of these the *precise interval* is *more*, not *less* than the number of whole years there specified; I shall subjoin one of these examples for the sake of illustration. Ptolemy there says, “that from an observation made by Timocharis on the 29th of the Egyptian

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tian month Athyr in the 465th year of Nabonassar, to another made by Agrippas in the 12th year of Domitian, on the 2d of the Egyptian month Tybi in the 840th year of Nabonassar—there intervened *between* them (μεταξύ) 375 years.\*” Now the *precise interval* between these two observations amounted in reality to 375 full Egyptian years and 33 days; for the 375 years would end on Athyr 29 (the 3d month) in 840, from which to the day of the second observation on Tybi 2d (the 5th month) are 33 days more; we see then that in this case the *precise interval* becomes *more* than the *gross interval* of whole years, because the 2d observation fell *later* in the Egyptian year than the first observation: but it still equally appears from this example, that Ptolemy never meant by μεταξύ any more than by μετα to express any thing except the *gross interval* of whole years, reckoned from the *end* of the year of the first observation to the *end* of the year of the second; which *indefinite* mode is not able of itself to determine, whether the *precise interval* will be *more* or *less* than the *gross interval*, until that *circumstance* be examined, whether the 2d observation fell *sooner* or *later* in the Egyp-

\* Τιμοχαρις αναγραφει, διότι τῷ ΜΖ εἴτει τῆς πρώτης κατὰ καλίππον περιόδου, κατ’ Αἰγυπτίους τῇ ΚΘ τοῦ Αδύρ το νοτίον μέρος ἡμισυ τῆς Σελήνης εἴφαινετο, &c.—καὶ ἐστὶν ὁ χρόνος κατὰ τὸ ΤΞΕ ἐτος ἀπὸ Ναβονασαροῦ Αδύρ ΚΘ—

Αγρίππας δὲ αναγραφει, ὅτι τῷ ΙΒ εἴτει Δομετιανοῦ ἡ Σελήνη ἐπεκαλύψε, &c.—καὶ ἐστὶν ὁ χρόνος κατὰ τὸ ΩΜ ἐτος ἀπὸ Ναβονασαροῦ κατ’ Αἰγυπτίους Τυβὶ Β—τοῦ ΜΕΤΑΞΕΥ τῶν δύο τήρησεων χρόνου περιέχοντος ἐπὶ ΤΟΕ.

This is the same as the 3d Athenian date referred to in my Tables.

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tian year than the first. The other example in the same page of Ptolemy is to the same purport as this latter one, therefore may be omitted; but this latter one serves moreover to clear up another subject liable to some doubt: for in both these examples as well as in the case of *μετα σπετη*, and in several others, which occur in Ptolemy, there is such a medley of dates by calippic and intercalated years and days with Egyptian and not intercalated years and days, that it might be doubtful, or at least might perplex readers to determine, whether by such phrases as *375 years between*, he meant 375 calippic or Egyptian years. Now in this latter example it becomes evident, that he could not possibly mean any but Egyptian years, because there is no calippic date annexed to the second observation in this latter example, but only an Egyptian one; and indeed universally throughout Ptolemy I know of no instance, in which he sums up any period by calippic or intercalated years, but always by Egyptian ones, and when he means to point out the *precise interval*, he always adds the odd overplus days to the number of whole Egyptian years: so that there is not the least reason to conceive, that by *μετα σπετη* (where the same medley of calippic and Egyptian dates occurs) Ptolemy meant full intercalated, that is, calippic years equal to Julian ones, but only retrograde Egyptian ones without intercalation, which *must* cause an *overplus* of 70 odd days *more* than 285 Egyptian years in the 2d sentence; but a *deficiency* of 295 days *less* than 285 years in the 1st sentence. Again, another similar example in which the

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*precise interval* was more than the *gross interval* of whole years, occurs in the first paragraph at p. 181 above; where Ptolemy says, *Εν τοις μεταξυ αρα της ὅλης διαστάσεως ΦΟΑ εἴσιν, &c.* for the two preceeding sums of 152 years and 419, out of which the *gross interval* of 571 years is composed, are exceeded by the *precise interval* by 140 days and a fraction, as Ptolemy himself there shews; and through the same cause as before, viz. because the second observation fell *later* in the Egyptian year than the first; but it might just as well have happened to have fallen *sooner*; and yet in both cases we have found, that he equally expresses the *gross interval* by *μεταξυ*: why then should he not have just as well meant to express only the *gross interval* by *μετα* in the case of *μετα σπε ετη*; and thus this will contain more than the *precise interval* in the first sentence, although less in the second sentence on account of the very same difference in *this circumstance*? So that whether he employs *μετα* or *μεταξυ*, he means uniformly the same thing, viz. *in* and *upon* the 285th or the 12th or the 375th or the 571st Egyptian year after the end of that year, *in* which the first observation was made; and thus by this popular mode of reckoning by whole years, he leaves the *precise interval* undetermined, and to be collected by readers themselves from the *materials* which his other dates afford them. Any other sense affixed to the above phrases, except that which I have pointed out, will introduce such confusion and contradiction, that it will be impossible to reconcile any one computation in Ptolemy with any other; while on the contrary the sense

above

above-mentioned produces *harmony* between all the computations throughout his whole work; nevertheless have the astronomers ever given the least attention to this? If they had, they could have never accused the *date* 178 of *inconsistency*, and I myself might have been spared the trouble of employing so many words; which some perhaps may condemn as superfluous, without considering that every ænigma appears to be very easy and obvious, when it is once found out: and that the right sense of *μετα σπε ετη* has hitherto appeared ænigmatical, is sufficiently evident from the consequences, viz. the many unsatisfactory reasonings which have been offered during two whole centuries, as well as the unjust accusations, to which that numeric phrase has given birth in regard to Hipparchus, together with the licentious conjectures and interpolations in Ptolemy's text.

Εν τῷ ΑΒ εἶπαι ἐπεσημηναι το μαλιστα]\* We come now to the *second* quotation from Ptolemy, in which he compares his own observations on the equinoxes with those of Hipparchus: and if any one should here think, that this word *μαλιστα* implies, that Hipparchus himself placed more confidence in this observation in the 32d year than in any of his others; he will perceive, that *μαλιστα* could never be intended to imply any such sense, if he does but look back to the foregoing paragraph, wherein he will find *μαλιστα* equally applied to *all* the observations of Hipparchus, Ταις τε ὑπο του Ιππαρχου μαλιστα ἐπισημανθεισαις ὡς ασφαλεσταται εἰλημμεναις ὑ' αὐτου. From what I have urged above it appears, how-

\* See p. 179. paragraph 2.

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ever, that Petavius may be so far right in what he has asserted in my notes to p. 138 and 182, as that Hipparchus had more confidence in his vernal than in his autumnal observations; but there is not the least reason for what Petavius after Albategnius and others has added, that Hipparchus thought better of that in the 32d year than of any other autumnal one; on the contrary, he confesses it to be in an error of 6 hours *too soon*, and indeed it could not be accurately observed at all on account of its falling in the night: nevertheless as he happened in the same 32d year to make his first vernal observation, this enabled him to correct his autumnal one by *computation* from the vernal one; and to fix it at *sun-rise* on Sept. 27; it still remains also a *doubt*, whether he has not fixed it nearly right. It has happened moreover by mere accident, that Hipparchus, and Ptolemy likewise in his extracts, have both of them made *more use* of this observation than of any other, partly, as I have shewn at p. 163, because there happened to be a total lunar eclipse in the 32d year, of which Hipparchus took advantage to observe the star *Spica* after Timocharis and others; and partly because from this 32d year to the observation of Ptolemy in the 463d Egyptian year there were about 285 years, which is *nearly one half* of the interval from Meto's observation of the summer solstice to that of Ptolemy in 463: for this interval contained 140 days more than 571 years; now twice 285 amounts only to 570; so that if Ptolemy had made use of the observation in the year 33, the *duplicate* of 284 would have been but 568. Hereby Ptolemy was

better able to compare the length of the solar year in those 285 years with nearly their duplicate 571, by thus making use of his earlier autumnal observation in the 32d year in preference to any *later* one out of the last three.

ΕΦ' ὅλοις Αἰγυπτιακοῖς ΣΠΕ ΕΤΕΣΙ. *Paragraph 4: p. 180.*] As these words, in which mention is here *again* made of a sum of 285 years, follow immediately after the account, which Ptolemy gives of the *gross interval* between the autumnal equinox of Hipparchus and his own autumnal one (which interval amounted in *precise length* according to the present dates in the text to no more than 284 years and 70 days) hence possibly former astronomers may have been induced and future ones may be equally misled, to alter *one* of those dates, *i. e.* 178 to 177, in order to extend the *precise interval* to agree with *this sum* of 285 full and *whole* years; for that 285 *full* years are meant *here* is sufficiently plain from the word ὅλοις: and they may erroneously conclude, that since 285 *whole* years are meant here by ΕΦ' ὅλοις ΣΠΕ ΕΤΕΣΙ, therefore the *precise interval* of 285 *whole* years *at least*, was equally meant before by μετὰ σπε ετη, and not the indefinite *gross interval* of 285 years, amounting by the accidental *circumstances* of the case to no more than 284 *whole* years and 70 days; it becomes necessary therefore to set the meaning and reasoning of Ptolemy in this passage in its right light, and in fact it will be found to give no colour whatever to the propriety of such an alteration of the date 178. For by these words in 285 *whole* years Ptolemy does not *recapitulate* or make a *second mention* of what the *above interval* was between

tween the two autumnal equinoxes; but he draws here in the present passage *a conclusion*, which follows from the *statement* he had before made of *that interval* in the two preceeding paragraphs 2 and 3; *the gross or general interval* was indeed expressed there by 285 years, but the *precise interval* was only 284 years and 70 days: Ptolemy being conscious therefore, that the *precise interval* in question was only a fractional part of 285 Egyptian years; he proceeds now by way of *conclusion*, to point out *here* (as the illative conjunction *therefore* (αρα) proves) how the case would stand in a sum *a little different*, namely, if he was to substitute a *precise interval* of 285 *whole* Egyptian years and 70 days, in place of the above *gross interval* of 285 years, which amounted to no more than 284 years and 70 days; and this he does in order to lay a more solid foundation for two subsequent conclusions which he proceeds to draw in regard to the still larger and rounder intervals of 300 years and 600 years. His reasoning then, when fully dilated, is to the following purport: "If *therefore* (says he) instead of the above *gross interval* of 285 Egyptian years (which accidentally contain only a fractional part of 285 such years) we take, as the foundation of our reasoning, in regard to the proportions we propose to institute, a *precise interval* of 285 *whole* Egyptian years of 365 days each, there will during this interval have been inserted in an intercalated year, like the Julian, 71 bissextile days with 6 hours overplus\*; so that 285 retrograde Egyptian years

\* For if 285 years be divided by 4, the quotient will be 71 and  $\frac{1}{4}$ , which shews therefore, that there will



with 71 days and 6 hours will be contained in 285 Julian years. Now it appears by the observation on the autumnal equinox by Hipparchus compared with my own, that from the hour of his observation of the equinox at *mid-night* on the 3d of the epagomenæ in the year 178 after the death of Alexander, down to my own on the 9th of the 3d month Athyr at 1 hour *nearly* after sun-rise, was included the *precise interval* of 284 Egyptian years and 70 days and 7 hours (i. e. a 4th part of a day and a 24th more, viz. 1 hour) \*. But Hipparchus at the same time allows there, that the hour at which he had fixed this *observation* of the equinox (if it can with propriety be called an observation when it fell in the night) was 6 hours *too soon*; and when he came to *correct* it by *computation* from the preceeding vernal equinox, which he had observed more accurately because it fell in the day, he found that his autumnal one could not have really fallen before *sun-rise*: thus then there were only 284 years 70 days and 1 hour from the *autumnal* equinox of Hipparchus, when *thus corrected*, down to my own observation of the same equinox in the Egyptian year 463†. Yet

be 71 bissestile days and 6 hours more in 285 Julian years intercalated with 1 day in every 4 years.

\* We shall see afterwards why Ptolemy instead of a 24th part made it the greater sum of a 20th part of a day, viz. Ἡμέρας τὰς πέντε, Ο και Δ και Κ εγγιστα μιας ἡμέρας.

† For from *mid-night* on the 3d of the epagomenæ (where Hipparchus placed his *observation*) there were 6 hours to *sun-rise* on the 4th of the epagomenæ, and there

between the *vernal* equinox observed by Hipparchus in the same 32d calippic year down to my own observation of the *vernal* equinox in the same Egyptian 463d year, there was an interval of 285 years and 70 days and 7 hours\*: since then the difference between the *two intervals* amounts only to *one* year and 6h. it is useless to make *two separate cases* and institute *two separate proportions* in consequence of this *small* difference; we will therefore augment the *first interval* between the two *autumnal* equinoxes with *one year* in order to make it equal to the *second interval*. Thus we will take 285 years instead of 284; and if my own observation had been thus made *one year later*, namely in 464 instead of 463, the autumnal equinox *must* in the following year 464 have fallen 6 hours *later* than I found it by observation in 463; so that the *first interval* would be augmented by 1y. 6h. and become 285 years 70 days and 7 hours, therefore exactly equal to the *second interval*: consequently in *both intervals* the difference would be exactly the same between 285 *solar years* (containing 285 Egyptian retrograde years 70 days and 7 hours) and 285 Julian intercalated years

there were 2 days more to the end of that year 178, and 284 years to the beginning of 463, in all 284 years 2 days and 6 hours; to which adding 68 days 1 hour of 463 to the 1st hour after *sun-rise* on the 9th of the 3d month Athyr, they make in all 284 years 70 days and 7 hours; but as the *correction* by Hipparchus placed the equinox at sun-rise 6 hours *later*, this reduces the interval to 284 years, 70 days and 1 hour.

\* As may be seen in paragraphs 5 and 6.

(containing 285 Egyptian years 71 days and 6 hours) and this difference would in *both* the above cases be 23 hours, that is, *one day wanting one hour*, i. e. a 24th part of a day.\*”

Such then is the nature of Ptolemy's reasoning, when all the *intermediate steps* are supplied which Ptolemy has *omitted* and left to his readers to supply, and which connect the *conclusion* in *paragraph 4* with the premisses and *state of facts* concerning the two observations as given in *paragraphs 2 and 3*; by which steps it clearly appears, that his *conclusion* follows *necessarily* from his premisses without any alteration or pretended correction of his text of any kind. This *fact* then being certain, that there is *no necessity* to alter his text in order to conceive *his reasoning* and to arrive at *his conclusion*, here a question arises, are we to prefer *this method* of supplying the *intermediate steps* between the premisses and conclusion, which I have pointed out above? Or shall we give the preference to *that method* of supplying *those steps*, which astronomers have adopted, and which requires an alteration of the date 178 into 177 against all evidence, as I have shewn, both internal and external? Now if we adopt the *method* employed by astronomers, we must stop our progress in the preceeding dilated paraphrase concerning the *intermediate steps* at these words in Italics there, “ 284 Egyptian years and 70 days and 7 hours :” but astronomers

\* Ὡστε προτερον γεγυνε ἡ αποκαταστασις της παρα το Δ επουσιας ἡμερα Α λειπουση το Κ μερος εγγιστα : why he says a 20th instead of a 24th part will be seen afterwards.



found that this *interval* differed *one* whole year from the interval, as given in *Paragraph 4*, contained in these words *επελαβεν αρα εφ' ὅλοις αιγυπτιακοις ΣΗΕ ετεσι ἡμεραις ταις πρῶταις Ο και Δ και Κ μιας ἡμεραις εγγισται*; for only 284 years &c are found above to intervene between the *dates* of the two observations in Ptolemy's text; and yet he reasons *here* in *Paragraph 4*, on a *supposition* of the interval being 285 years, &c. What then is the remedy which the astronomers have provided against *this disagreement* in order to be able to connect Ptolemy's *conclusion* with his *premisses*? It is by supposing the *text* to be corrupted, and that the date of the observation of Hipparchus ought to be 177 instead of 178; thus they would *extend* the interval by *adding 1 year* to it at its *beginning*. But can rational men approve such a licentious and violent mutilation of the Greek text, when astronomers might, along with myself, just as well have *added* that *one* deficient year at the *end* of the interval? and this, not by supposing the text to be corrupted, and that the date 463 ought to be 464; but by supposing, that in *Paragraph 4* Ptolemy *intentionally* made an addition of *one* year to the *end* of the interval 284 &c, in order to make the interval between the two *autumnal* equinoxes in the first case *equal* to the subsequent interval in *Paragraph 6* between the two *vernal* equinoxes in the second case, that he might thereby not make *two* cases of two intervals so *nearly equal*: for the addition of 1 year could produce no *sensible* difference in *observation*, and consequently no *material* difference in the *proportions*, which he proceeds to prove to be necessarily resulting

from *those two intervals* \*. This latter method then of supplying in Ptolemy's *reasoning* the intermediate steps, which are necessary to connect his *conclusion* with his premises, seems to be much the most probable, even if we had no other evidence in its favour than mere *supposition*; and what other evidence than mere *supposition* have the astronomers for their own *corruption* which they have now adopted for 200 years? But if *supposition* be set against *supposition*, surely that must be preferred, which does no violence to the text and yet arrives at the very same end of proving, that Ptolemy's *conclusion* does certainly and necessarily follow from his *premises*; and which proves it likewise by such simple and reasonable means, as only conceiving, that Ptolemy would prefer to reason on the foundation of the *correction*, which such an ornament of science and human nature as Hipparchus

\* For Ptolemy there proves, that according to the observations of Hipparchus and himself, the difference between 285 solar years and 285 Julian years amounted to no more than 23 hours; consequently at the same rate the difference between that *one* solar year which Ptolemy *added* and one Julian year could not amount to 5 *minutes*, which was too small a difference to become *sensible* in observations of the equinoxes, concerning which Hipparchus had confessed, that he could not be sure of the accuracy of his observations within 6 *hours*; it was therefore quite *immaterial* with respect to the *proportions*, which Ptolemy proceeds to institute: and accordingly we shall see afterwards, that he often passes over such *minute* and *immaterial* differences without taking the least notice of them, and even of much *greater* differences.

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had himself made in *his observation*, rather than on the *observation* itself, which had been both confessed and proved to be in an error of 6 hours *too soon*: and which in fact cannot with propriety be even called an *observation* at all; because the equinox fell in the *night*, when the sun could not be observed by the instrument called the *Armilla*, employed by him for that purpose. Much more reasonable must it be to prefer *my supposition*, if it be considered likewise to what vast and unreasonable consequences the opposite *supposition* of the astronomers leads us; which indeed are even of such a nature, as Geometricians would style a deduction *ad absurdum*. For the alteration of the *date* 178 into 177 does not end there; but it moreover necessarily follows thence, that the calippic period must have begun at the summer solstice, and then all the 6 autumnal observations must have been made *one whole year sooner* than otherwise they would be: now this variation not only renders those observations so very erroneous, that all the learned world has hitherto been astonished at it from such a man as Hipparchus, but it also makes those *errors* to stand in direct contradiction to the *words* of Hipparchus; who expressly says, that several of those observations fell 6 hours *too soon*, whereas the above variations make them actually fall above 6 hours *too late*; But thus his own *words* affirm, that they ought to have fallen *still* 6 hours *later*. Nay still farther, 4 or 5 other *dates* or *sentences* in Ptolemy must undergo similar alterations to that of 178 into 177, either in the reading of the *text* or conceiv-



ing its *sense*; and these, as I have shewn, in direct contradiction again to antient evidence, and to every probable construction, as is proved by the common meaning of Greek words and by parallel passages even in Ptolemy himself. What an Herculean then as well as unnecessary labour have astronomers undertaken, a greater one than even that of Atlas in sustaining the world upon one of his shoulders! for he only sustained one world, but modern astronomers have likewise attempted to form another as vast, and a totally new ingenious world of their own to bear upon the other shoulder. However, fortunately my *better method* of connecting the reasoning of Ptolemy, which I have pointed out, is not in reality a *mere supposition* (as the corruptions of Ptolemy's text by astronomers most certainly are, and very desperate suppositions likewise) for the word *ὅλως* contains, as I mentioned before, a very strong *indication*, that Ptolemy did in reality make *here an addition* of 1 year to the *preceeding* interval; this word *ὅλως* is, I believe, nowhere employed by Ptolemy, except with strict propriety, that is, to set any *whole sum* of which he speaks in the stronger opposition to a *part*, or the several *constituent parts* of it mentioned in his preceeding sentences: *ὅλως* contains, therefore, a strong though tacit *indication*, that a *whole sum* of 285 years is here in *parag.* 4. set in opposition to *that part* of it which is contained between the preceeding dates of the two observations in *parag.* 2 and 3; and which, as Ptolemy was conscious, amounted only to 284 years, 70 d. to which *part* when he added one

one year, they would constitute together, what he calls, the *whole sum* of 285 years, 70 days.\*

\* For example, at p. 181, Εν τοις μεταξὺ ἀρα τῆς ὅλης διαστάσεως ὅσα ἐτεσιν, &c. Now he had just before mentioned the *two parts* of this period to be 152 years and 419; out of which he then adds the *whole* period of 571 years is composed. So again soon after, φανερον ἀρα, ὅτι ἐν ὅλοις τοις χ' ἐτεσιν, &c. here likewise he had just before mentioned *one part* of this period, viz. 571 years; from the circumstances attending which he draws a *conclusion* concerning what the *state* of things *would be* in a *whole* period of 600 years. Thus in like manner, as I contend, in the present example of εφ' ὅλοις σπε ἐτεσι, &c. he draws a *conclusion* how the case *would be* in this *whole precise* period of 285 years and 70 days, since the circumstances *actually were* such as he had just before *stated* them to be in a *part* of that *precise* period, viz. in 284 years and 70 days: which latter period although according to the popular mode of expressing *gross periods* by *whole years* without fractional days, he had just before *called* indeed 285 years in the popular phrase μετα σπε ετη, yet on account of the concomitant circumstances the *precise interval* amounted in *reality* to no more than 284 years and 70 days; and it has been the neglect of astronomers in not observing this, and not thus reducing the *gross interval* expressed by that popular phrase to its real and *precise interval*, which has caused all the mischief and mistakes here concerning the meaning and reasoning of Ptolemy. For it could no more be expected, that Ptolemy should have said here in *direct words* "Observe, that in my phrase εφ' ὅλοις ἀρα σπε ἐτεσι, I add 1 year to the preceeding interval 284," than that afterwards in the passage, φανερον ἀρα ὅτι ἐν ὅλοις τοις χ' ἐτεσιν, &c. he should have said likewise in *direct words*, "Observe, that I add 29 years here to the preceeding interval 571." All the *intermediate steps*, which connect the *conclusion* with the preceeding *facts* are equally omitted in both cases;

Although then Ptolemy has not said in *direct words*, that in the sum expressed by his phrase  $\epsilon\phi' \delta\lambdaοις \sigma\pi\epsilon \epsilon\tau\epsilon\sigma\iota$  he has added 1 year to the *first precise interval*, as *stated* before in parag. 2 and 3, yet at least the word  $\delta\lambdaοις$  contains a strong *indication* of such a tacit addition being there made; which astronomers themselves would have easily discovered, if they had not taken up a groundless opinion, by their mistaking the import of the phrase  $\mu\epsilon\tau\alpha \sigma\pi\epsilon \epsilon\tau\eta$ , that the *precise interval* between the two autumnal equinoxes *ought* to have been found *equal* to 285 years. Hence in their enquiries how it came to pass, that this interval *is* in reality found to be only 284 years according to the *dates* in the present text, they have all stoppt short and contented themselves with *this supposition*, that the present text is corrupted, and that this supposed *deficiency* of 1 year in the interval happened through a corruption of the date 177 into 178: whereas if they had looked a little farther into the reasoning of Ptolemy, they would have seen the real truth, that it was astronomers themselves who were mistaken, and that they *ought* to have never made any such *supposition*, as that the

cases; and it never would have been questioned, whether *one* year was not tacitly added in one case, as well as 29 years in the other, if it had not unfortunately been an addition of *only one* year; and moreover if it had not again unfortunately happened, that the popular phrase  $\mu\epsilon\tau\alpha \sigma\pi\epsilon \epsilon\tau\eta$  *seemed* (though erroneously) to imply, that the first interval amounted to 285 years originally and without any such subsequent addition of 1 year, when in reality that general phrase contained a precise interval of only 284 years and 70 days.

*first*



*first precise interval* ought to have been found by the dates *equal* to 285 years; consequently that it was not *accidentally* shortened 1 year by any corruption of the text, but that Ptolemy himself, when he came in paragraph 4 to draw his *conclusion* from the *facts* before stated, did *intentionally* make an addition of 1 year to the preceeding *first precise interval*, in order to make that *first interval* between the two autumnal equinoxes *equal* to the *second interval* between the two vernal equinoxes, which *really* amounted to 285 years and 70 days. Neither is it to be wondered at, that these paragraphs should be thus capable of *this* more probable and consistent explication, if it be considered, that they have hitherto appeared to be so very perplexed according to the *common* explication of the astronomers, that Christmannus honestly thus confesses the fact, "Intervallum annorum (viz. 285 & dierum 70) certissimum est, licet obscuris verbis a Ptolemæo declaretur." p. 342.

If in opposition to this fact, that Ptolemy did in *paragraph. 4* make such an *addition* of one year to the *first interval*, it should be still asked, *why* did he rather thus increase 284 to 285 than reduce 285 to 284? The answer to this will be easy. For it is very evident, that it would be of no use to make *two cases* and institute *two different proportions* in consequence of these *two intervals*, which differed only one year: now it was more convenient for Ptolemy to augment the smaller interval 284 by one year rather than to subtract one year from the greater interval 285, for the reason already given at the bottom of p. 219; viz. because 285 was, more nearly

than 284, *one half* of the larger interval of 571 years from Meto's observation to that of Ptolemy, as there shewn\*. This alone was a very sufficient reason; but there was still another, viz. because he could hereby with greater accuracy conform *the proportion*, which he institutes, to the larger and rounder sums of 300 and 600 years; for 285 has exactly the same proportion to 24 hours wanting 72 minutes (i. e. a 20th part of a day) as 300 has to 24 hours; and the *nearest duplicate* of 285, viz. 571y. 140d. 6h. has *nearly* the proportion to twice 24 hours wanting 2h. (i. e. a 12th part of a day) as 600y. has to twice 24 hours, i. e. 2 days, the duplicates of 300 and 1 day. These circumstances then by shewing that it was not without good reasons that Ptolemy preferred to augment 284 to 285 rather than the contrary, contribute at the same time to *confirm* the reality of the *fact*, that he actually *did* so, and that he *intentionally* made such an *addition* of 1 year to the first interval 284; so that it did not differ from the 2d interval 285 through any *accidental* corruption of the dates in the text, but because the two intervals never were originally equal, nor would

\* That observation is said by Ptolemy in *paragr. 7.* to have been made in the archonship of Apseudes at the summer solstice; this was *in* the 316th year of Nabonassar, which ended on Dec. 9: from the *end* of that year to the *end* of 424, with which the æra of Nabonassar and Alexander ended, are 108 years; and these added to the 463 after Alexander, make up 571 years to the *end* of the 463d year after Alexander on July 20; at the summer solstice *in* which year Ptolemy made his own observation.

have

have become *equal*, except by means of this *voluntary* addition made by Ptolemy himself in paragraph 4, on account of the sum 285 being more favourable than 284 to the accuracy of the *proportions* founded on it, which he immediately proceeds to deduce and point out in regard to the rounder and larger intervals of 300 and 600 years.\*

As long rooted errors generally resist their own remedies with obstinacy, it may, perhaps, be still urged in their defence, that it is no way probable, that Ptolemy should do such an act as that of adding *one whole year* to the *real* interval, at least not unless he had expressly mentioned the same in a more explicate and evident manner. To this that reply is sufficient which I have made above already, namely, that in fact the *difference* of a single year *more or less* in the interval, was in this case a matter, which drew after it neither any *sensible* nor *material* consequence; because it could not produce a varia-

\* I may here notice another circumstance, which confirms this tacit *addition* of 1 year; which is, that in *paragraph 4* he omits the *definite* article τοῖς; for he says ἐφ' ὅλοις, &c. and not ἐπὶ τοῖς ὅλοις, &c. this difference might form the same different senses, as in the two following English phrases, "therefore in a sum of 285 years," and not "therefore in *the* sum of 285 years," *i. e.* in *the* above-mentioned sum of 285 years; this latter phrase he avoids here, and prefers one *consistent* with the following sense, "therefore if we were to make up *the* above interval equal to a sum of 285 years." Whereas in the second interval in *paragraph 6* he inserts τοῖς and says τοῖς σπε εἴεσιν, *i. e.* in *the* above-mentioned 285 years, for so many they really were.

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tion in the *proportions* deduced from that interval to the amount of 5 minutes. But it not only thus appears from *Theory*, that this small difference of *one year* could not be worth Ptolemy's pains to mention in a more particular manner than he has done; it does moreover appear likewise from Ptolemy's *own practice* soon after, that it was no unusual thing with him thus to omit taking *the least notice* whatever, either of the precise amount of any such sum *so added*, or of such *small differences* in the length of intervals, as could produce no *sensible* nor *material* difference in the conclusions which he drew from those intervals, and that he considered them as mere *nugæ*. For example, from the foregoing two intervals of 285 years each between the equinoxes, Ptolemy drew this conclusion, that in every period of 300 years, 300 solar years were shorter than 300 Julian intercalated years by 1 day. Now in *paragr. 7* he proceeds to prove the same fact from the *duplicate* of 285, 70d.i. e. the interval of 571 years 140 days between the solstice observed by Meto and that observed by himself; and he concludes thence again, that in every period of 600 years, 600 solar years were shorter than 600 Julian years by 2 days. Thus we see that he arrives at the *very same conclusion* and *proportion* by means of *both* those intervals; but the latter longer interval of 571 years was not an *exact duplicate* of 285 years; for twice 285 years and 70 days amount only to 570 years and 140 days: so that here again the larger period *exceeded* the *duplicate* of the smaller one by *one year*, just as the *second interval* of 285 *exceeded* the *first interval* of

284 by *one* year; and yet Ptolemy does not take the *least notice* of this *difference*, because *this* again was so small a one as could produce no sensible variation in his conclusion. Hence we find, that it was agreeable to *his own practice* actually to omit *all notice* of such small differences as these, and *Theory* justifies the propriety of his practice. Nay, in the subsequent *paragraph* 8 Ptolemy gives us another similar example, although the *difference* there instead of *one* amounts even to *five* years: for he observes, that the interval between an observation of Aristarchus on the solstice and another by Hipparchus was 145 years, and yet he there proves Hipparchus himself to have concluded from a comparison of these two observations, that those 145 solar years were shorter than 145 Julian years by 12 hours; which is in the *same proportion* as one day in 300 years, notwithstanding that 145 want 5 years of 150, *i. e.* of one half of 300 years. Nevertheless Ptolemy takes *no notice* of, nor makes the least account of or abatement in the proportion for *this difference* of 5 years, because it could not cause a variation of 25 minutes; which small difference could not become *sensible* in observations of the solstices and equinoxes, wherein they all thought themselves fortunate if they did not fall into an error of 6 hours. We have no room to be surprised then, that Ptolemy should in *parag.* 4 by a tacit and voluntary addition of 1 year, reason upon *the supposition*, that the immediately preceeding interval was 285 years (although it was really only 284) because which ever the sum was, it made not the least material difference in his  
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conclusion; but 285 was preferable to 284 on other accounts; for it was the *real* interval in the subsequent case of the two vernal equinoxes, and it was moreover *necessary* to the scrupulous accuracy of the subsequent proportion, viz. that 285 is to 24 hours wanting a 20th part (72 minutes) as 300 is to 24 hours or 1 day.\*

There still remains one other objection, which may be made to the paraphrastic explication, which I have given of this 4th *par.* at p. 221, &c. and this I have reserved to the last, because it is the only one, which has the least shadow of force in it; but which will, I think, be found to be only a mere shadow, and not to have any real substance. In that paraphrase I have supposed Ptolemy to reckon the overplus 70 days and a 4th and a 20th part of a day, not from the hour of the *observation* by Hipparchus at *midnight*, but from the hour of the *correction* made

\* It may be here asked, how came Ptolemy to make this fractional part to be a 20th part (i. e. 72 minutes) when we found above, that it amounted to no more than 1 hour, which is only 60' or a 24th part of a day? The reason is, because his own observation was not made precisely at one hour after sun-rise but *nearly* (εγγιστα): Now did he by *nearly* mean a little *less* or a little *more* than one hour? It appears by his own words afterwards, that he meant a little *more*; for he says, "that he found the interval to contain an overplus (above 70d.) of 6 hours and a 20th part of a day." i. e. ἡμέρας 70 καὶ ὃ καὶ κ' εγγιστα μίας ἡμέρας, paragraph 4. So that his observation must have been made at 12 minutes *more* than 1 hour after sun-rise, which added to 60 minutes make up 72 minutes, viz. a 20th part of a day.

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in that observation by Hipparchus, viz. at *sun-rise* 6 hours later than the *supposed* time of the observation, I say *supposed*, because it must have been fixed in a very precarious manner, if the equinox really fell in the night. If then the hour of *actual observation* (if it can be so called) was so very precarious, is it not more *probable*, that Ptolemy reckoned those *overplus days and hours* from the more certain hour of *correction* than from the more uncertain hour of *observation*, especially when it was a correction made by such a man as Hipparchus? and moreover, even if Ptolemy was in ever so much doubt whether to prefer the *observation* to the *correction*, yet another circumstance would have determined him in favour of the *correction*; for as it was *necessary* for him to fix on the one or the other to begin his computation at, he would in course prefer the *hour of correction*, because thereby (after his addition of 1 year) 285 solar years would be shorter than 285 Julian years by exactly *the same* quantity, (viz. 1 day wanting a 20th part) as well in the *first* interval between the two autumnal equinoxes as in the *second* interval between the two vernal equinoxes. But to *these probabilities* it may perhaps be still opposed, that Ptolemy has *actually said*, that on the contrary he reckons the sum from the hour of *observation*. Let us examine then whether this assertion be true. All that there is to countenance it, is, that when in *parag. 2.* Ptolemy *recapitulates* or quotes a *second time* his former account of this observation by Hipparchus, he makes mention of the hour of *observation* at *mid-night*, but not of the hour of *correction* at *sun-rise*, Του μεσονυκτιου

του εις την Δ φερωντος—here he stops short, without adding again a *second* time δεον πρωτως, as in his *first* quotation of these words in *observ.* 4, at p. 178: and on account of this *omission* it may be thought, that he reckons afterwards from the hour of *observation*, which he *does* here mention, and not from the hour of *correction* which he *does not* mention. This circumstance *may indeed* at first be thought to contain some admissible evidence of that fact; but it ought to be considered farther, that his omission of δεον πρωτως *may also* have been merely accidental; for all writers every day find by experience, that being themselves conscious of the connected train of reasoning in their own minds, they are hence apt sometimes to omit a necessary link of information to others, and without which readers are at some loss to comprehend their meaning: it *might* moreover have appeared to Ptolemy not *necessary* to quote δεον πρωτως a second time, as he had already quoted it with accuracy only a page and half before. Besides, it is evident still farther, that in this *recapitulation* he does not scrupulously attach himself to the very *same words* as before; nor yet to the very *same arrangement* of them, why then might he not take a similar liberty of omitting to quote the *whole* of them? For at p. 178 the words are, Της Γ των επαγομενων εις την Δ του μετονυκτιου, δεον πρωτως: but in *parag.* 2. we read Τη Γ των επαγομενων του μετονυκτιου, του εις την Δ φερωντος. So that in this latter quotation he might have never intended to make a *second complete* one, but only to quote *enough* of it to enable his readers to refer back to his former quotation of the same observation; and thus merely to ascertain which of the former obser-

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uations he had here in view: it might then be much the same here as in a quotation from Ptolemy, ascribed by me to Theon in my note to p. 146; where a reference is made to Ptolemy's words in the following still more brief manner, Εὐ μὲν γὰρ τῷ ΑΒ εἶται—this imperfect quotation we should be now apt to supply in some degree by the addition of an &c. as in fact I have actually supplied it there for the more easy comprehension of my readers, although &c. is not in the original Greek. Ptolemy then in like manner might mean to refer to the *whole sense* of his former quotation, inclusively of δέον πρώτως, although he has not recapitulated the *whole words*. But lastly, if Ptolemy's *omission* did neither happen by mere *accident* nor *oversight*, nor by such an intended *brief reference*; yet it may be questioned, whether in fact there really be any such supposed *omission* here of the *sense* of δέον πρώτως, notwithstanding that those *words* themselves are indeed omitted: for he has at the same time *added* the word φερόντος which is not to be found in the first quotation; and this might have been intended to *supply* the *general sense* contained under δέον πρώτως, or at least as much of it, as he wished to retain. For φερόντος may mean something *more* than merely *tending to the 4th day*, it may mean also *extending more or less toward the 4th day*\*; that is, Ptolemy might intend to express by this word, that he would

\* Thus Isocrates says, Εξω φερεσθαι τῶν καιρῶν, *To be actually carried away from the objects which opportunity offered*: and διαφερεῖν means sometimes, *actually to draw away toward different sides*. Vid. *Additions*.



not take upon himself to determine, whether the *correction* by Hipparchus was to be preferred to his *observation*; therefore he placed the equinox at some intermediate point in the interval *extending* between mid-night and sun-rise: thus Φεροντος would include as much of the *sense* of δεον πρως as Ptolemy thought proper to be included, and thereby it accounts for the *omission* of those *words* themselves. But although Φεροντος *may* have this *sense* here, yet I do not affirm, that it actually has; this however I may observe, that I cannot meet with a single example any where else in Ptolemy, where Φεροντος is employed as here, in conjunction with such words as εις την Δ: I did not indeed notice the *addition* of this word in this sentence early enough to attend to every observation recited in Ptolemy's work; but I have since endeavoured to refer to every one of his observations, and at worst to the greater part, without being able to meet with any one instance whatever of Φεροντος being employed by him in such a situation as here. Does not this afford a *presumption*, that it was not *added* here at random as a mere *expletive*, but intentionally, and as conveying some *peculiar* and useful meaning? and such a meaning we perceive it is able to convey. It is indeed true, that such phrases as the following often occur in Ptolemy's observations, Ημερας τη γ του μεσσηνικου εις την δ, that is, he often informs us, *that an observation was made on the 3d day (or some other) at the mid-night followed by the 4th day*; or in a still more general way, "at some intermediate point or other of the night of the 3d day, which was followed by the 4th day." In all these

these cases the words *εἰς τὴν Δ* seem to be altogether *useless expletives*, at least to such of his readers as know, that the Egyptian day began at sun-rise; but they might be *useful* to those, who were accustomed to begin their day at midnight; for in this case one half of every night *preceded* and the other half *followed* the same nominal day. What Greek verb in these cases is *omitted* and understood in regard to such phrases as *εἰς τὴν Δ*, I will not pretend to determine, but I have never found *ὑπονοεῖται* inserted any where except in the present case under our consideration; which circumstance renders it the more remarkable, and excites a strong *presumption*, that its insertion here was intended to answer some *particular purpose*, and what that purpose *may be* I have also pointed out. *All* then or *any* one of these considerations seem to be sufficient to remove the force of the objection started above to the accuracy of my explication of Ptolemy's reasoning in my paraphrase of *parag. 4.* and to the important as well as numerous consequences dependant upon it: yet this is the *only objection*, which has occurred to me against that explication. But is such a trivial objection as this on one side (if it has still a right to be called an objection) to be set in competition with the numerous and insuperable objections, which I have made against the current opinions and explication of modern Astronomers on the other side? For I have shewn, that in order to make good their own opinions, they have been forced to mangle and new model Ptolemy's Greek text from the beginning to the end with respect to the *words, dates, sense, or arrangement,*

against the faith of all manuscripts, all antient quotations from him, and all grammatical propriety respecting the Greek language; and *every one* of their arbitrary alterations is so intimately connected with the rest, that if *any one* of them be received, they must be *all* received, there is no medium; *aut Cæsar aut nullus*. Whereas according to my own explications and the opinions connected with them, we have not found the least occasion to alter a single *word, date, or letter* in any one quotation, of which we have *made use*; nor yet to give any other *sense* to Ptolemy's words, than such as either *necessarily* belongs to them, or else is agreeable to the *practice* of Ptolemy elsewhere, or at worst actually are their *natural* senses in other authors, and may be their *real* ones here. So that hereby *harmonious consequences* follow each other throughout the whole work of Ptolemy as naturally as the shadow follows the substance. Could this be possible, if my explications were all this time involved in a labyrinth of errors? Can it be expected, that a long series of errors should be all of such a fortunate kind, as by a happy jumble to coalesce harmoniously with each other, and thus to assume the false appearance of truth? As well might it be expected, that the atoms of Epicurus should, by a fortuitous concurrence, be able to produce the consistent ends and magnificent scenes of the creation.

Having then thus fully *explained* and also *justified* the meaning of Ptolemy and the nature of his reasoning in the paragraph where ἐφ' ὅλοις ὅπερ αἰγυπτιακοῖς ἐτέτι occurs, it may be expedient still farther to observe, that this is *one* of the



the *two* passages referred to at the bottom of p. 150, which might possibly be produced by some persons to prove, that neither the *date* 463, nor yet *that* of 285 in the phrase *μετὰ σπε ετη* could have possibly undergone any corruption. But we now find, that the integrity of *μετὰ σπε ετη* will *not* be vindicated by the words *εφ' ὅλοις σπε ετεσι*, for if the former had been originally *μετὰ σπε ετη* (284) yet *εφ' ὅλοις σπε ετεσι* (285) would have remained exactly the same: because in the latter passage Ptolemy makes an *addition* of one year to the *former interval* 284; nevertheless I do not suppose *μετὰ σπε ετη* to have undergone any such corruption, because it is *a mode of computation* although not *a phrase* agreeable, as I have proved, to Ptolemy's universal method of expressing a *gross interval* in his other observations; but it is however the *very phrase* likewise employed by Hipparchus just before. The *other* of those *two passages* referred to at p. 150 is more to the purpose for which it may be produced; for it does in reality prove the integrity of the *date* 463, and that Ptolemy's observation must have been made in that year 463, not in the later year 464; it is subjoined below\*. He says here, "that since his observation of the autumnal equinox fell on the 9th of Athyr &c, and his vernal observation on the 7th of Pachon, &c there were 178 days and 6 hours

\* Ἡ μετοπωρινὴ ἰσημερία γέγονε τῇ θ' τοῦ Ἀθύρ, ἡ δὲ ἐαρινὴ τῇ ζ' τοῦ Παχών, ὥς συναγεσθαι τὴν διαστατὴν ἡμερῶν 178, δ. *Lib. 3. c. 4.*

between the two."† Hence it is evident, that his autumnal *preceeded* his vernal observation, otherwise instead of 178 days 6 hours, there must have been 187 days between the two. Now as his vernal observation was certainly made in the year 463, which ended on July 19, therefore his observation of the autumnal equinox, by immediately *preceeding* his vernal one must have been made in 463 likewise. Thus then we find no error, nor any reasonable cause for suspecting any error in *any one* of the dates relative to this question, and by our thus finding nothing throughout *but harmony*, this is at the same time a proof both of the *integrity* of the text and of the *rectitude* of those explications, which I have given of it.

But hence we find in truth, and a sad truth it is, that those boastful philosophers physical and metaphysical, many of whom daily set themselves up as infallible guides to the public in religious opinions, do nevertheless in their own proper spheres of knowledge, read, write and reason with just as much negligence and even ignorance as the rest of mankind: like the il-

† Here another proof offers itself, how unsolid and precarious those arguments are, by which Bouillaud and M. de la Lande pretend to have proved that 178 ought to be 177 (see p. 206, 7, 8) for *here* we find the *right* interval preserved between the autumnal and vernal observations of Ptolemy on the equinoxes; and yet astronomers agree, that they were *both* in an error of *one* whole day: so that the error in the one must have been proportionable to that in the other; but it might *not* have been the same in those of Hipparchus.

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literate multitude also they daily build up incoherent systems in science, religion and politics upon very crazy foundations, and they often argue most learnedly and most extravagantly concerning the causes and consequences of facts, which never had any existence except in their own wild imaginations. What then is all this but romance, and the very worst kind of romance, because it contains not even amusement any more than instruction? But on the contrary, it misleads and deceives the public into errors, to which men would have been neither prone from their nature nor their reason, if they had not been thus first confused and distracted by false representations of pretended knowledge under the ostensible garb of science and erudition: for various other similar *specimens* and *proofs* of these assertions see my *Appendix to Vol. 4.* against Dr. Priestley. Such then is the spirit of what now assumes the imposing appellations of philosophy and metaphysics; and of which it may be said with too much truth along with Cicero, “Nescio quomodo, sed nihil est tam absurdum, quod ab aliquo philosophorum non sit dictum.” Yet these are they, who take upon themselves to dictate to others in theology and christianity! Blind guides, who strain at a gnat and swallow a camel! I esteem myself happy then, that a sufficiency of health and leisure has been indulged to myself, to set this *astronomic romance* in its true light before the eyes of mankind; which although almost as antient as the *historic one* of Geoffry of Monmouth, yet can scarcely boast of being as true: from the *specimen* however here given of this visionary bubble  
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formed by fanciful views of Ptolemy's *text* or *meaning*, my readers will be better able to judge how easy it is for philosophic and metaphysical speculation, when assisted by dexterity in the deceitful art of words, in the abuse of reason or facts, and in the extravagance of wanton suppositions, to invent various chimerical and inconsistent systems of Christianity, like the Arian one of Dr. Price, or the Unitarian system of Dr. Priestley; and then to father them all upon the Gospel as containing its true and genuine meaning: along with Selden they may all as confidently say, *non uno in loco certissime liquet ex Evangeliiis*; and along with Dr. Preistley they may all swear, that they have read to the same purport in the christian fathers likewise; when in fact nothing tending to confirm such discordant opinions is to be found either there or elsewhere, nor ever had any existence whatever, except in the whimsical theories, or the party prejudices, or the false representations or incoherent roving of their own heads. Thus these sophistical arts of disputation, under the plausible pretence of *free enquiry* and the solemn name of *philosophy*, those boasted Panacæas for all the ignorance of mortality, are in fact found by experience to lead men into new errors and greater distresses than the blindness of human nature was ever destined to inherit; until it had been thus perplexed and distracted by this mode of artificial chicanery, to the manifest obstruction of real knowledge, to the reproach of human reason, and to the disturbance of society in those moral blessings, which *christianity* and *fair enquiry* were sent to bestow upon the poor,

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naked, ignorant, helpless and wretched race of man; either as he exists in a mere state of savage nature, or of long accustomed habits of profligacy, or even under any mode of civil government: for indeed this latter, through the absurd contrariety and multiplicity of its laws, has generally proved, at best, but a heavy oppressive burden upon the necks of mankind, tolerated merely in order to ward off the still greater evils arising from the violence of human passions. So that by no method whatever can the miseries of mankind be removed, except through such *rational precepts* as are recommended by the mild spirit of christianity, and by a firm union in *religious belief* as a sanction for such plain home-felt moral truths; the same can neither be effected by means of the intricate subtleties of a *disputatious philosophy*, nor yet by the endless labyrinth of *civil laws*. Here then we may discern the true cause of a fact, which has lately struck all ranks of men with horror, that is, the long lists of criminals yearly increasing at every assizes; for the true cause is, the relaxation and neglect of all religious, civil and moral discipline in the lower ranks of men, together with the subversion of all principles in the higher ranks by false philosophy in many, and by the luxury and extravagance of fashionable manners in all. Of the dangerous tendency of such *false philosophy* it may therefore be justly said, "Her throat is an open sepulchre, her house inclineth unto death; none that go unto her, return again in safety, neither take they hold of the paths of *truth* any more; for when they have forsaken the religious guide of their youth,

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and forgotten the covenant of their God, with much fair speech she causeth them to err, with the flattering of her lips she enticeth them; then go they straitway after her as a fool to the correction of the flocks; alas! she hath thus cast down many that were wounded, yea, many a strong man hath been slain by her, when they had left the paths of upright evidence to walk in the ways of dark conjecture; for their ways indeed are dark and crooked, and they froward in their words."

Lastly, as at p. 211, I have pointed out a parrallel passage in Ptolemy, where the *precise interval* is *less* than the *gross interval* of the 12 whole years there expressed by his phrase τοις IB ετεσι μεταξυ, and therefore the case may be the same with that other *gross interval* expressed by μετα σπε ετη (viz. 285 years) but containing in reality no more *precisely* than 284y. and 70d; so now I shall proceed to point out another parrallel passage in Ptolemy to confirm the sense, which I have affixed to the phrase Επελαβεν αρα η αποκαταστασις εφ' ολοις αιγυπτιακοις ΣΙΠΕ ετεσι: and I shall prove both by a like expression and reasoning of Ptolemy elsewhere, that this *conclusion* which he draws from his prior *statement* of the *dates*, is a conclusion concerning a period of years *exceeding* the *precise period* (included between the dates) by *one year*; and that in both passages little other notice is given by him of this his tacit *addition* of *one year*, except what is contained in this phrase itself επελαβεν αρα &c. Now since the different senses, in which I have understood the two sentences μετα σπε ετη and επελαβεν αρα &c from those affixed to them by astro-



astronomers, form the foundation of our different opinions and of the different reasonings ascribed by us respectively to Ptolemy; hereby the foundation of my own will be secured in a solid manner, by its resting upon the actual meaning of Ptolemy himself in the parallel passages here thus adduced by me, as well as by the natural and current meaning of such phrases in other Greek writings. Let us quote enough of the passage now in question just to render it intelligible to readers in a popular manner; and as for those who wish to enter more minutely into the subject, they may have recourse to the original. The title of the chapter which I propose for our present consideration is, *Concerning the correction of the periodic motions of the Planet Mars*; which he begins with the following words, according to the translation of Trapezuntius: “Sed gratiâ etiam emendationis periodicorum mediorum motuum unam cepimus de priscis observationibus, quâ declaratur, quod anno 13 secundum Dionysium capricornionis mense 25, stella Martis matutina cernebatur boreali Scorpionis incumbere fronti, & est tempus observationis in anno 52 a morte Alexandri, hoc est annus 476 a Nabonaffaro, die 20 Athir secundum Ægyptios sequente 21 in mane. — Quoniam igitur anni 409 fuerunt ab observatione usque ad Antoninum &c.”\* Here we

\* Και της διορθωσης δε ενεκεν των περιδικων μεσων κινήσεων ελαβομεν και των παλαιων τηρησεων Α, καθ' ην διασαφειται, οτι τω ΙΓ ετει κατα Διονυσιον Αιγωνος ΚΕ, εως ο Αρεος τω βορειω μεταπω του σκορπιου εδοκει προστεθειναι· ο μεν ουν της τηρησεως χρονος γινεται κατα το ΝΒ ετος απο της Αλεξανδρου τελευτης,

see that Ptolemy himself reckons up only 409 years from that *antient* observation referred to in the 476th year of Nabonassar to the reign of Antoninus, which soon after he calls *the 1st of Antoninus* [κατὰ τὴν ἀρχὴν τῆς Ἀντωνίνου βασιλείας] this began with the 461st year after the death of Alexander, to which adding the 424 before Alexander's death, they make up 885; from these deducting the 476 after Nabonassar, that is, the date of the above *antient* observation, there remain 409, from the *end* of that year to the *end* of the first year of Antoninus, for the *gross interval* in question. After Ptolemy has thus by way of *premisses* made a *statement* of what the dates are, which comprehend between them this *gross interval* of 409 years, he proceeds to a long detail of *reasonings* and *demonstrations* deduced from the above *premisses* or *statement* of the two dates and the length of the period between them; this he continues without intermission through a whole folio Greek page, and then finishes this subject and chapter with drawing at last the following *conclusion*: " Sed demonstratum etiam fuit, quod in tempore tertiæ oppositionis distabat secundum inæqualitatem ab eadem maxima epicycli longitudine gradibus, 171, 25: addidit ergo in interjecto inter observationes tempore (quod quidem 410 Egyptiacos annos & dies 231 hor. 8 proxime continet) post 192 integros circulos, addidit

τελευτῆς, τοῦτ' ἐστὶ κατὰ τὸ ΤΟΓ ἔτος ἀπὸ Ναβονασσαρου κατ' Αἰγυπτίους ΑΔυρ RK εἰς τὴν ΚΑ ορθρου\* ὥστε ἐπεὶ πάλιν τὰ ἀπὸ τῆς τήρησεως μέχρι τῆς Ἀντωνίνου βασιλείας ΤΘ ἔτη ἔσσι.

(inquam) gradus 61, 43\*." Although then Ptolemy at the *beginning* of this chapter *states*,

\* Εδεδείκντο δὲ ἡμῖν καὶ ἐν τῷ χρόνῳ τῆς τρίτης ἀκρονύκτου κατὰ τὴν ἀνωμαλίαν ἀπέχων τοῦ ἐπικυκλοῦ μοίρας ROA, KE· ΕΠΕΛΑΒΕΝ ΑΡΑ ἐν τῷ μεταξὺ τῶν τήρησεων χρόνῳ (περιέχοντι αἰγυπτιακὰ ἔτη TI καὶ ἡμέρας ΛΑ, Γ ἐγγύστα) μέθ' ΟΛΟΥΣ κυκλούς R5B, μοίρας ΞΑ, ΜΓ &c. *Lib.* 10. c. 9.

Here I may observe again, that the errors in Ptolemy's text are chiefly concerning the days of months and sums of days. For example, in the *first* passage 120 (RK) must be an error for 20 (K) arising from a duplication of R on account of the preceeding word Αδύρ ending with R; this is proved by the subsequent sum KA: accordingly Trapezuntius and others allow this correction in their translations. There is also one, if not two errors, in the dates of the *second* passage; the text reads ΛΑ, Γ i. e. 31 days and  $\frac{1}{3}$  of a day; but the real number of odd days between the 1st observation and the 2d is 231; so that Σ has dropt out of the text on account of the preceeding word ἡμέρας ending with a Σ, and we should read σλα (231): This Trapezuntius and others allow likewise. But there must be still another error in the fraction Γ,  $\frac{1}{3}$ , for this would be only 8 hours, whereas the real number of odd hours were 16; accordingly to this purport Trapezuntius translates the text, yet has 8*b.* only on the margin. For the 2d observation was 2 hours *before* midnight, to which date are 16*b.* from 6 o'clock the morning, therefore  $\frac{2}{3}$  of a day; hence Trapezuntius renders the text  $\frac{4}{3}$  i. e.  $\frac{2}{3}$  of a day: so also Averanus. But this cannot agree with the Γ in the text, yet none of them inform us how it is to be corrected. I apprehend, that a Δ may be flipt out, and that the whole should be σλα, Γ, Δ, i. e. 231*d.* with  $\frac{1}{3}$  and  $\frac{1}{3}$  equal together to 14*b.* viz. 8 hours and 6 hours. For Trapezuntius has erroneously rendered ορδρου in the 1st observation by *in mane*, in the morning, that is, at or after sun-rise, the general word for which in Ptolemy is πραιας; but ορδρου means



that the *gross interval* between the two observations amounted only to 409 years, yet here at the *end* of the chapter, when he draws his final *conclusion* from all his foregoing reasonings, he supposes and says, that the *precise interval* between the observations amounted to 410 years and 231 days; which is impossible, if the *gross interval* amounted to no more than 409 years: here then again, just as before in the case of the observations on the equinoxes, his *conclusion* applies to a period, which *exceeds* the period contained in the *premises* by *one year*; and it is remarkable, that he draws his *conclusion* in both passages in nearly the same phrase,  $\text{Επελαβε}$

means *diluculo*, during the *twilight*, therefore before sun-rise, yet possibly only a little before, which at that season of the year would deduct 2 hours *nearly* ( $\epsilonγγιστα$ ) from the 16h. For the 476th year of Nabonassar began on Oct. 30; so that the 20th of the 3d month Athyr (the 8th d.) would coincide with the 17th of Jan. when the sun rises very little before 8 o'clock, therefore nearly 2 hours after 6 o'clock; consequently this *deficiency* of 2 hours in the 1st observation must be deducted from the *supernumerary* 16h. of the 2d observation; for Ptolemy expressly says that the hours in the 2d observation were *horæ æquales* ( $\iotaσημεριναι$ ): these 14 hours would be expressed by  $\Gamma, \Delta$ , i. e.  $\frac{1}{3}$  and  $\frac{1}{4}$  of a day, viz.  $\frac{35}{48}$  of a day. Just as he says, that a certain eclipse must have happened at 50 minutes after midnight, i. e.  $\frac{50}{60}$  of an hour, which he expresses by  $\Sigma, \Gamma$ , viz.  $\frac{1}{2}$  and  $\frac{1}{3}$  of an hour, 30' and 20' [ $\text{οφειλεν γεγονεναι προς } \Sigma \text{ και } \Gamma \text{ μερους μιας ωρας του μεσονυκτιου}$ ] lib. 3. c. 5. If Ptolemy had meant 16 hours, as Trapezuntius and Averanus suppose, he would have used either his customary word  $\deltaιτριτον$ ,  $\frac{2}{3}$  of a day, or else the notation M, 40 i. e.  $\frac{40}{60}$  of a day. Vide the note p. 254 below.

αροι

ἀρα ἐν τῷ μεταξύ των τηρήσεων χρόνῳ—μεθ' ὧν  
 κυκλῶς R<sup>5</sup>B &c. Now may not a hasty reader be  
 here again induced to pretend, that there must  
 be some error in the text? May he not here  
 again have recourse to *conjectural emendations* and  
 rashly assert, that either at the *beginning* of this  
 chapter we ought to read 410 or else only 409  
 at the *end* of it? This he might do with just  
 as good a grace as Copernicus in altering 178  
 to 177; for in both passages, if we examine  
 the progress of Ptolemy's reasoning more mi-  
 nutely, we find no sufficient foundation for sup-  
 posing any error in the text: that there is no  
 good foundation for the former correction of  
 Copernicus I have proved already, and the same  
 will be equally seen in the present parallel case.  
 For here again Ptolemy *almost* as tacitly as before  
 makes an *addition* of *one year* to the *interval* con-  
 tained in the *premises*, only with this difference;  
 that when before he *added one year* to the inter-  
 val between the two autumnal equinoxes, he  
 looked *forward*, so as to make it equal to the  
*subsequent* interval between the two vernal equi-  
 noxes: whereas in the present passage he looks  
*backward* so as to make his *conclusion* apply to  
 the interval between that *antient* observation  
 above-mentioned, and another which Ptolemy  
 had mentioned two whole chapters *before*, as  
 made by him in the 2d year of Antoninus, there-  
 fore *one year later* than as *stated* in the *beginning*  
 of this chapter; so that if there were 409 years  
 to the 1st of Antoninus, there must be 410 to  
 his 2d year. Thus we find, that after a whole  
 page of demonstrations relative to the interval  
 of 409 years as stated in the *beginning* of 9th  
 S chapter

chapter to be the *premisses* from which he sets out in his course of reasoning; yet he comes at the end of that chapter to make his final *conclusion* refer to a *different interval* from that with which he *began*, and *exceeding* it only by *one year*, but without having ever mentioned this interval before *throughout* that whole chapter: so that *inter observationes*, at the end of the chapter do not mean the *same two observations* as *ab prisca observatione usque ad Antoninum* at the beginning of the chapter, but the same as *ab prisca illa observatione usque ad secundum annum Antonini*; which observation in the *second* of Antoninus had not been so much as mentioned by him in any *later* passage than twice in the second chapter preceeding the *present* one now under consideration; in which preceeding chapter he says, that he should make use of *three* observations of the planet Mars, of which *the third* was made “anno Antonini secundo Epiphi secundum Egyptios die 12 sequente 13, ante mediam noctem duabus æqualibus horis.” c. 7 *apud initium*.<sup>\*</sup> This observation, he afterwards in that *preceeding* chapter, calls *the third opposition* of Mars, and it is to this distant observation in chap. 7, that

\* Την δε Τρίτην τῶν Β' ἐπεὶ Ἀντωνίνου, καὶ Ἀργυρίου, Ἐπιφ. Β' εἰς τὴν ΙΓ' πρὸ Β' ὥραν τοῦ μεσονυκτίου. Lib. 10. 7. Here again Ἐπιφ. Β' is an error for Ἐπιφ. ΙΒ', caused by Ἐπιφ. ending with an ι, as is proved by the subsequent sum ΙΓ'. Such errors in the text as these are, we see, generally accompanied with concomitant evidence of their being errors. The same date is also repeated at the end of this chapter, and there the text has Ἐπιφ. ΙΒ'.



his words and *conclusion* refer at the end of chap. 9, when he says, "*demonstratum fuit quod in tempore tertiæ oppositionis &c.*" it is then in the interval from this *third opposition* back to the antient observation in the 476th of Nabonassar that there intervened those 410 years and 231 days here in question. Now if Ptolemy could with so little information on his part as only by the obscure words *in tempore tertiæ oppositionis*, make his *final conclusion* from all his long demonstrations in chapter 9 refer back to an observation mentioned at the distance of two long chapters before, and differing only by one year from his *statement* of the dates of the two observations at the *beginning* of this very chapter 9 in question; why might he not equally in the similar phrase *ἐπελαβεν αὐτοὶ ἐπὶ σπτε εἴστοι* refer, not back to a statement of an interval of 285 years between the dates of the two autumnal equinoxes 178 and 463, but forward to the immediately subsequent interval between the two vernal equinoxes, which actually amounted to the 285 whole years expressed by *ἐφ' ὅλοις σπτε εἴστοι*? And in fact the word *ὅλοις* contains almost as strong evidence, that Ptolemy did not look back to the foregoing *fractional* interval of 284 y. and 70 d. as the words *tempore 3tiæ oppositionis* do, that in regard to the 410 years he did look back to the distant observation in the 2d of Antoninus, which he had mentioned so long before. There is the more reason also for this presumption, because in the case of the 284 and 285 years his reasoning necessarily becomes ambiguous on account of his having omitted all the *intermediate steps* between his

*premisses and conclusion*; whereas in regard to the 409 and 410 years all the *intermediate steps* are inserted, yet so scattered however throughout the extent of three whole chapters, that the *end* of the 9th chapter is not, as we see, intelligible without looking *back* to the *beginning* of the 3d chapter before it, any more than ἐφ' ὅλοις σπετεσι is clearly intelligible without looking *forward* to the full 285 years and more between the two observations in the immediately subsequent sentence.

P. 10. "*The Macedonians continued until the very last to employ the old octennial period.*" ] Dodwell and others allow, that the Lacedemonians always retained the octennial period, yet the proofs which he offers of this fact are nothing so clear and conclusive as those I have produced concerning the Macedonians, "*Periodum laconicam octaetericam fuisse faciunt ut suspicemur veterum testimonia.*" p. 317.

P. 20. "*Africanus computes the 490 years in Daniel by the octennial period.*" ] The octennial period had been laid aside at Athens on the introduction of the Metonic 650 years before the age of Africanus, what then could induce him, in explaining the times in Daniel for the use of the Greek christians in Asia, to employ such an antiquated period if it had not been still in use among the Asiatics? one might as well suppose, that a modern commentator would write in the old style of Chaucer in preference to that of Addison. To which I may add, why  
should

should Eudoxus, about 50 years after Meto, have corrected the octennial period, if it had not been still in use?

P. 33. "*Not any one of those 4 authors has found all the four Athenian dates to agree with their tables.*" ] But all the four Athenian dates in Ptolemy do agree with my own tables, and the two last of the Macedonian ones likewise; the first of these however begins 1 day too soon in my tables for Ptolemy's date; which is perhaps an argument, that these Macedonian dates were not reckoned by the calippic period, but by the octennial or some other mode.

P. 37. "*Again, whenever &c falls later than the day actually marked &c.*" ] Read—*actually found* by the tables when thus corrected.

P. 43. "*Thallus might have been a spectator of the eclipse*" ] As a possible proof that he really was so, I may observe, that there is an inscription, dated under Tiberius, among the Arundel marbles, the 56th in Chandler's edition; in which one Alexander Azinienfis preserves from oblivion on marble the names of the friends of his youth at Athens, among which is found the name of Thallus.

P. 45. Line 5, for *make* read *object*.

P. 52. Line 9, "*Must have fallen so late in the day, &c.*" ] Read, "*Must have fallen so late in the day, which preceeds that marked in the tables, that it could not be visible before the following day, namely, not before the very day marked in the tables.*"

P. 60. "*Such an unastronomical time, as Scaliger somewhere calls it.*" ] I now find, that these are the words of Dr. Kennedy in his *Discussion of*



*Points in Chronology*, p. 19 : but I still think, that Scaliger had somewhere said the same.

P. 63. "*The exact time requisite to elapse before such another synchronism could possibly happen.*" ] By *the exact time* I meant, and my readers, as I presume, must have understood me to mean, not *the exact time* according to the greater accuracy of the moderns, in computing the precession of the equinoxes and the mean period of the moon, but *the exact time* according to such reputed accuracy as we have any reason to suppose current in so early an age of astronomy as 867 years before Christ. For when I contend against M. de la Lande, that the Chaldæans were not so ignorant as he supposes in regard to the length of the solar year, it does not follow, that they were as accurate as the moderns, either in regard to the period of the moon or the precession of the equinoxes ; but probably they were at a medium between the two, and had advanced about as far as the Greeks in their most flourishing state, therefore not farther than Calippus in making the several lunar periods contained in 76 years exactly equal to 76 Julian years. As to the precession of the equinoxes Calippus knew nothing of it ; Hipparchus who lived 150 years later and under the constellation of Rome was the first on record who discovered it ; so that the Chaldæans as well as Greeks apparently supposed the equinox to be stationary in the same point of the heavens. In regard to the sun, since its apparent motion is constant and regular, long observation might with the Chaldæans supply the place of the accurate instruments of the moderns ; but the motion of  
the

the moon is so very irregular, that nothing but the Newtonian principle of gravity can ascertain computations concerning it with accuracy. Now by the principles of the calippic period there would be a new moon again after the expiration of 473040 years or within 3 days; for if 473040 be divided by 76 there will be 16 remainder, which contain 2 octennial periods, therefore at the end of those 16 years there must be a new moon within 3 days later in reckoning downward and 3 days sooner in reckoning backward: but an error of 3 days is a mere nothing in so vast a sum; for if almost the smallest error was made in the length of the moon's mean period, when it became multiplied by so vast a sum, it would produce an error of so many days as would derange all computation. This would be another reason for the Chaldæans multiplying 47304 by 10, for at the end of 47304 years there would, according to *the same* principles, be no new moon until 7 days, a full quarter, afterwards; but, when multiplied by 10, within 3 days. So that 473040 years would be a *magnus annus* or great period, after the *expiration* of which there would be a restitution of the sun and moon into the same situations as at the *beginning* of it, according to the best astronomic principles, of which any account has been preserved previous to the age of Hipparchus. But observe, that I have supposed the length of the solar year according to the Chaldæans to be 365,5,48',53"; it would be a small matter less than 53" in order to produce 473040, when multiplied as pointed out in my *Appendix*.

P. 73. In note, "*Une eclipse fut observée.*" }  
 Read as in the original, "*Une eclipse de soleil*  
*fut observée;*" for an eclipse of the moon was  
 likewise observed by Theon, this however was  
 also in 364.

Ibid. "*Par. 1645, p. 334.*" ] Read "*Pro-*  
*legomena, p. 14.*" To these authors I may now  
 add Riccioli in *Almagest. nov. Bonon. 1651, p. 369*  
*of vol. 1;* who says, "*In 365 solis eclipsis fuit,*  
*anno Nabon. 1112 die 22 Payni mensis, ut re-*  
*fert Theon in commentario, fuit die 10 Martii.*"  
 Thus he has made the matter still worse, for he  
 not only mistakes the right year, but the right  
 month, the right day of the month, and even  
 the right calendar by which Theon dates the  
 eclipse in the month Payni 22; which was not  
 the retrograde Egyptian year, but the fixed  
 Egyptian year which began at the conquest of  
 Egypt by Augustus on August 29, and was  
 called κατ' Ἀλεξανδρεας, not κατ' Αἰγυπτίους as  
 the retrograde year was. In the latter ca-  
 lendar the 22d of Payni would in that year  
 have coincided with March 10, but in the  
 former calendar with June 16, the true day of  
 the eclipse in 364. Theon gives us the right  
 date by both calendars, but Riccioli by a strange  
 kind of cross-purposes transfers the date of the  
*month* by one calendar to the date of the *year*  
 by the other, and thus makes a monster of the  
 whole. Would these learned men have done  
 thus if they had applied themselves to Chrono-  
 logy? Nay if they had even consulted their  
 own science of astronomy, they would have  
 found no solar eclipse in 365 either on March 10  
 or June 16. Thus we see, that even physical  
 phi-



philosophers can not only with as much negligence as other men, but even with scientific confidence make assertions directly against the face of plain *truth*, just as our modern philosophical and metaphysical Unitarians do in every page directly against the *truth* of history, chronology, language, grammar, and every part of speech! And if any one of them tells an idle and false tale, the rest will most certainly copy it, and even make it worse; *mala fides crescit eundo*. The words of Theon concerning this eclipse are these, κατ' Αιγυπτίους τῷ α, ρ, ι, β ετει απο Ναβονασαρου εν τῷ κδ του Θωδ — κατὰ δὲ Αλεξάνδρειας εν τῇ κβ του Παυνι. *Comment. Lib. 6.* Now the former of these two dates coincided with June 16 in 364, as I have shewn in *Crit. Observ. vol. 2. p. 43*; and that the latter date coincided with June 16 likewise, will obviously appear, if any one only reckons up the number of days from Aug. 29 inclusively to June 16 in the next year, they will be found to be 292: but *Payni* being the 10th month, if the 9 times 30 days of the foregoing 9 months be added to the 22 of *Payni*, they make up 292 likewise. In case there be a difference of 1 day, it arises only from the different places of the bissextile in the Julian and Egyptian calendars. What a puzzle then have all the philosophers made about a very clear case! and yet it has never hitherto been set at rights now at the end of 150 years! Can we wonder that the same persons should mistake the meaning of Ptolemy's words concerning the observations of Hipparchus on the equinoxes, who have so notoriously mistaken the meaning of Theon's words concerning the

the dates of a solar eclipse? What hopeful interpreters, therefore must philosophic critics be of the sense of words in scripture either in Greek or Hebrew, who have shewn themselves to be so very intelligent and accurate in their own sphere of science concerning the plain Greek words and dates both of Ptolemy and Theon? In fact they seem to make no more difficulty in exchanging the right and wrong days, months and years in those authors, nay even whole calendars, cycles and periods, than a conjuror would do in changing his cups and balls. Yet such is the state of physical *truth* in this present boasted age of *learning*, and of a similar nature is the degree of *either*, to which our Unitarians also have attained! In fine, if I was to collect together all the notorious mistakes, extravagant suppositions, romances, blunders and absurdities of physical, metaphysical and Unitarian doctors, it would form an instructive lesson to the pride of philosophy to teach it the true value of christian humility, and even to teach it the true value of christian history and tradition, which with all their imperfections are no way more liable to error, than the speculations of philosophers have been in all parts of knowledge. This then is that philosophy, which at present charms all mankind, and which would but too often have been thought an enemy to reason and fidelity, if it had not been sanctioned by a name, which like christian charity, is able to cover a multitude of sins! It is to be wished therefore that all persons would read more attentively, conjecture more cautiously, argue more accurately, and

and write less hastily, that if they do not promote, they may at least not obstruct the progress of knowledge; which defects although in other sciences they may tend only to prolong the darkness of ignorance, yet in matters of religion they excite ill-founded doubts and disputes, discord, animosities, indifference to all religion and every evil work. Simple error and ignorance may be but the natural lot of humanity, but the artificial chicanery of *false knowledge* is the disgrace of literature, the bane of society, and the death both of reason and religion. If such be physical philosophy, what judgment must we form of her metaphysical sister? Voltaire seems to have thus given her a true character, "*Vanitas vanitatum est metaphysica vanitas*;" and there is no occasion for any farther evidence of the justness of this censure than the following passage in a late tract by a learned and acute judge—"If I were to give a definition of the *transcendently* noble science of metaphysics, I would call it the art of puzzling, or a science which teaches how to confound *reason*: indeed, when I find one great metaphysician, Dr. Berkley, proving, that there exists no *matter*, and another great metaphysician, Dr. Priestley, proving, that there exists no *spirit*, and thus between them reducing the whole universe to a miserable *void* (not to mention a thousand other metaphysical extravagancies which this century has hatched) I am tempted to insert in my litany *a metaphysicis libera me Domine* \*." By a similar train of sophistical

\* Dr. Geddes in *General Answer* to criticisms on his  
new



disputation Dr. Priestley is attempting likewise to prove, that orthodox christianity now holds the directly contrary tenets, to what it did originally. Well might the Greeks, when they came to their sober senses, expel sophists from society! In the name of common sense can such romancing prolusions as these be called *learning, science and philosophy*? Rather let all those who form any pretensions to such acquisitions, take *truth* for their object, and *reason* for their guide, otherwise their learned labours can be thought of in no better light than as solemn trifling; and yet like children they still daily go on to build up houses with cards, which the first puff of wind blows down again: but

The dog-star rages, and 'tis past a doubt,  
All bedlam or philosophy's let out.

POPE.

P. 85. In note, “*αποδημῶντες.*”] It is in the *third* account, which Herodotus gives of the Scythian invasion, that the above word occurs, viz. in *Lib. 4 apud initium*; and by this word employed there he clears up a doubt, which subsisted in his two former accounts in regard to the epoch, from which the 28 years there mentioned are to be reckoned. See also *Crit. Observ. vol. 2. p. 181.*

P. 86. “*Not far distant from the right year of his reign.*”] For Eusebius places this expedition of Cyaxares against Niniveh in his 13th year, and according to my tables it was at no

new translation of the Bible, p. 24. 1790: A work which deserves better encouragement.

greater

greater distance than his 15th and might *possibly* be even in his 14th year, if we allow that he advanced against Niniveh in the *very same* summer, at the spring of which he made peace with Alyattes, viz. soon after March 5.

P. 94. "Others may be discovered hereafter."] Add, "Accordingly another Chaldaean period of 432000 years is mentioned by Syncellus p. 30, and this also is found by M. Baily to be still subsisting in the astronomy of the Bramins in India; it is the length which they attribute to the last of their 4 ages of the world, i. e. the present one, called *Calougan*. See *Astron. Orientale Disc. prelim. p. 168* and *p. 111*, where M. Baily points out the astronomic origin of this long period.

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*Additions and Corrections in the above Illustrations.*

P. 103. "Ληγοντι may signify &c] That ληγοντι in Ptolemy, or in general elsewhere, never expresses a more precise idea than the Latin phrase *labentem annum*, i. e. that the year is *on the decline*, may be confirmed by the following further examples. Plutarch in his life of Timoleon informs us, that this commander of the Greek army in Sicily, gained his great victory there over the Carthaginians ληγοντι μηνι Θαρργηλιωνι: if then we can ascertain on what day of Thargelion this battle was fought, it will illustrate the meaning of ληγοντι. Now Plutarch happens also in his life of Camillus, to mention the days which had been lucky or otherwise to the Greeks, and among others he  
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notices that the victory of Timoleon was gained on the 24th of Thargelion [τῇ ἑβδόμῃ φθινοντος] as this was the 7th day before the end of the month, there was therefore *almost* a full quarter of the month unexpired; why then should not Ptolemy also employ ληγοντι in a similar case, when *almost* a full quarter of a year was still unexpired? And that there is no error in the text of Plutarch in regard to the day in Thargelion, appears from what he adds, that it was on the same day, on which Troy had before been taken by the Greeks, according to Ephorus and others.

Nay, Ptolemy himself, likewise, affords us an example to confirm, that ληγοντι was actually sometimes, at least, employed by him in the same extensive sense as by Plutarch, and which is indeed its proper sense. In *Lib. 7. c. 3.* he says, "That Timocharis made an observation of the moon on the 29th day of the Egyptian month Athyr, at the 3d hour [of night] *declining*," [κατ' Αἰγυπτίους τῇ κθ' τοῦ Αἰθύρ ὥρας Γ' ληγουσης] and soon after he further ascertains the *precise time*, by adding, "that the observation was made at 3 hours and 1-3 of an hour before midnight; thus we find, that ληγουσης means here a full third before the 3d hour was expired [προ Γ και Γ (δια το τον ἡλίον περι τας 3 μοιρας ειναι του ὑδροχοου) και προς τας ὁμαλαι νυχθημεραι σχεδον προ τούτων, ΩΡΩΝ ΤΟΤ ΜΕΣΟΝΥΚΤΙΟΤ ΣΥΝΑΓΕΤΑΙ ὁ χρόνος] " ante mediam noctem tribus temporalibus horis & equalibus 3h. 20' (sol enim in 7 gradu Aquarii erat) colligiturque tempus etiam ad dies æquales ante mediam noctem fermè horis illis." So Trapezuntius translates the



the passage, but the words in italics are not in the Greek, they are the translator's own addition; whether they are an erroneous addition (as I conceive they are) I leave to others to judge; and shall only observe, that by  $\pi\rho\omicron$   $\Gamma$   $\kappa\alpha\iota$   $\Gamma$   $\omega\rho\omega\nu$   $\tau\omicron\upsilon$   $\mu\epsilon\sigma\omicron\upsilon\nu\eta\tau\iota\omicron\nu$  is meant, as the Greek text now stands, 3 hours and  $\frac{1}{3}$  (20') before mid-night, therefore a full third before the third hour was expired; and it should seem that Ptolemy must mean here *horæ temporales*, because in 4 or 5 immediately subsequent and similar observations, he in like manner gives the dates first by *horæ temporales* and then informs us to what sum of *horæ æquales* those were equivalent. If there be any error or omission in the Greek text, that may make an alteration in the conclusion, but I see no room to suppose any error or omission.

P. 136. "The autumnal equinox fell on Sept. 27, &c." That is, according to the current construction, which the astronomers put on the words of Hipparchus and Ptolemy; but according to my own explication the equinox fell on Sept. 26: yet this makes no difference in regard to the present computation; because as the equinox fell a day sooner, so the Egyptian new year fell a day sooner likewise.

P. 146. "Theon fails us here." I now find, that Theon's Comment. on this 3d book is lost, and that it was supplied by Nicolaus Cabasilas archbishop of Thessalonica about the year 1200; but still the presumption contained in it against there being any error in the Greek text remains the same, yet the evidence is here-

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by carried no farther back than to the 12th century.

P. 153. In note, "There the calendar of Calippus might be in vulgar use." *Add*, "This then may be the reason, why in the only *four* instances in Ptolemy, wherein he mentions any Greek month along with a calippic date, we find them to be always Athenian months, namely, in the *four* Athenian dates marked in my tables: So that Athenian months might have been employed by Ptolemy in the calippic period, not as Chronologers suppose, because it was in common use at Athens (of which no testimony is extant, and it is directly contradicted by Diodorus, as may be seen above at p. 100, in note) but because they might be the months generally made use of at Cyzicum and in the Propontis, where the period of Calippus might have been adopted as a popular calendar.

P. 160. In note, "Corrected in the later editions." I now find, that there never was but one edition of the Greek of Ptolemy's *Almag.* viz. in 1538, at Basil; but there were 3 editions of the Latin translation of Trapezuntius; viz. in 1528, 1541, 1551; the latter ones differ in some respects from the 1st edition at Venet. 1528, of which 1st edition *Weidler* in his *History of Astron.* seems to have had no knowledge; and it is valuable on this account, that it was made before the Greek edition in 1538, and from a different MS. namely, from one in the Vatican library, as mentioned in the preface: the two later editions were printed at Norimbergh. The still more antient Latin edi-

edition of 1515 was made from the Arabic translation, not from the Greek.

P. 239. To the note add " Virgil and Cæsar employ in Latin the phrase *viam illuc ferentem*, to mean *the road leading or extending to such a place*; and Stephens in his Lexicon shews, that the Greeks employed *φερωνται* sometimes in the same sense, as, *Οδὸς εἰς Ἀθην φερουσα*. Hence it more plainly appears, that *φερωντος* in this passage of Ptolemy may not mean merely *followed by or pointing at* the 4th day (in order to shew those readers, who were accustomed to begin their day at mid-night, that he meant *the mid-night* near the end of the 3d day, and not *the mid-night* which began the 3d day) but that *φερωντος* was employed here on purpose to express that (as he could not determine whether the observation of Hipparchus was or was not preferable to his correction of it) he therefore supposed the equinox in question to have happened at *some* intermediate point or other, more or less, in the interval of 6 hours *extending* between the *observation* at mid-night and the *correction*, which fixed it in the subsequent morning. Thus *φερωντος* becomes a substitute for *δεον πρωιας*, and accounts for its omission, by implying as much of the sense of *δεον πρωιας*, as Ptolemy judged to be proper. His computation nevertheless might begin at the morning, because he was under a necessity to fix on some *precise hour*, at which to begin that, although he was uncertain whether he was quite right or not; but by the above qualifying phrase he guarded against the dilemma as well as he could do.

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## E R R A T A.

P. 109. In note, for "Lib. 2. c. 18 read c. 19."

P. 183. Line 3, for "Ptolemy's instrument read the instrument of Hipparchus."

P. 199. In note, for  $\Delta$ ,  $\epsilon$  and  $\zeta$ ,  $\epsilon$  read  $\zeta$  in both places. N. B. a  $\zeta$  is always in Ptolemy the mark of  $\frac{1}{2}$  or  $\frac{3}{4}$ : Query, is this mode of notation to be found in any other Greek MSS. or printed books? *Vid.* the same *err.* p. 181.

P. 201. Add to note there, "Hipparchus had not the same means in his power to correct his 1st and 2d autumnal observations, as he had to correct his 4th, 5th and 6th; because he had not made any vernal observation before that in the 32d year, the same year wherein his 4th autumnal one was made."

P. 204. In note, *lin. penult.* for "sooner than Calvisius" read later than Calvisius.

P. S. Dr. Priestley says, "I have waited in vain for the re-appearance of the author of *Critical Observations*—as he has had sufficient time, I must conclude that inclination is wanting; whether this has arisen from a consciousness of want of ability to fulfill his engagement with the public, must be left to the conjecture of our readers; for with respect to this also he is silent." *Defence of Unitarianism for 1789, Pref. p. 9.*—I cannot sufficiently express my astonishment, that Dr. Priestley can thus give himself up to continual error, sophistry, and misrepresentation of facts and truths both antient and modern. The real fact is, that I have already, in my *Appendix* to vol. 4, performed all that I ever engaged for, namely, to support, with evidence, what I had advanced in my *Discourse*, i. e. that both the Ebionites and all other sectaries of the first two centuries, were believers in the subordinate divinity of the Christ. If I have been since silent on this subject, it is, because I have found nothing in Dr. Priestley's *Replies*, which was worthy of my notice or of the author himself. As to any hints given by me of extending my views farther, they were only hints, which however I have not relinquished, but hitherto these other subjects have engaged all my time; and it were to be wished, that other philosophers, as well as Dr. Priestley, had not reasoned so rashly and writ so hastily; whereby I should have been spared this trouble of exposing their want of truth and fidelity: but I shall never have inclination to sacrifice the plan of this work, so as to render it a mere vehicle of altercation with such writers, as manifest no other wish, than only to perplex and confound the reason of mankind with unfolid disputation, in order to give a plausible appearance to the prejudices of a religious party.

